
DEO·ET·REGI

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Considerable Parts of the WORLD.

VOL. LVI. For the Year 1766.

LONDON:

Printed for L. DAVIS and C. REYMERS,
Printers to the ROYAL SOCIETY,
against *Gray's-Inn Gate*, in *Holbourn*.

M.DCC.LXVII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they

 should

ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of nature or art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons, through whoſe hands they receive them, are to be conſidered in no other light, than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

CON-

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TO

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XVIII. *An*

CONTENTS.

of

PHILO-

1765	rue Time.	Par. of the Microm.	Parts of a gr. Circle.	Mag. of he Ec. Dig.	
Aug. 16	h ' "		' " '''		
	0 45 0	2484	31 41 0	—	Diameter of the Sun measured in the Parallel.
	3 52 12	2486	31 42 30	—	Diameter of the Sun.
	3 58 13	—	—	—	Beginning of the Eclipse to a Second. The Sky clear about the Sun.
	4 7 57	2316½	29 33* 0	0 20	Magnit. of the Eclipse.
	4 9 57	969	12 21 30		Distance of the Horns.
	4 13 33	2200	28 3*3		Magnit. of the Eclipse.
	4 15 0				The Sun covered.
	4 20 2	2144	27 21* 0		Magnit. of the Eclipse measured without a Glass, light Clouds.
	4 22 3	1275	16 16 0		Dist. of the Horns, measured without a Glass, light Clouds.
	4 25 27	2063	26 19*		Magnit. of the Eclipse measured without a Glass.
	4 28 0				Thick Clouds covering the Sun.
	5 20 0				The Sun appears again, but the Eclipse is over.

The quantities marked with * are the remaining bright parts of the diameter of the sun measured in a direction perpendicular to the line of the horns.

II. *Remarks on the* Palmyrene *Inſcription at* Teive. *In a Letter to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society, from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Good Sir,

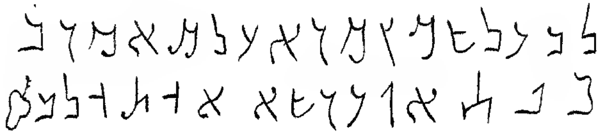
Read Jan. 23, 1766.

THE Palmyrene inſcription at Teive having been inaccurately taken by Sig. Pietro della Valle, the tranſcript publiſhed in the *Philoſophical Tranſactions* muſt be looked upon as incorrect, and conſequently the explication of that inſcription, which the Royal Society did me the honour (1) formerly to publiſh, cannot in all points be intirely depended upon. Having therefore been informed, that the ſtone itſelf, brought a few years ſince out of the Eaſt, was in the poſſeſſion of the Right Honourable the Earl of Beſborough; I reſolved to attempt getting a ſight of it, that a true copy of ſo curious a monument might in proper time be imparted to the learned world. Having opened my deſign to John Wood, Eſq; member of parliament for Brackley in Northamptonſhire, a gentleman of great merit and erudition, he carried me, with the utmoſt politeneſs and good nature, to Lord Beſborough's houſe in Cavendiſh-Square, May 21, 1764; where I had a full view of the ſtone, examined the in-

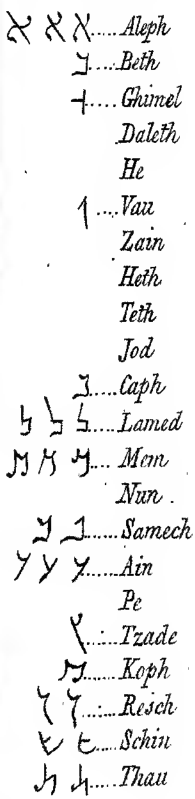
(1) *Philoſoph. Tranſact.* Vol. XLVIII. Par. II. p. 746-751.

ſcription

The Palmyrene *Inscription brought from* Teive.



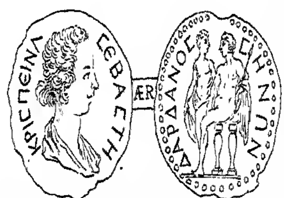
The Alphabet deduced from the Inscription at Teive.



Taken from the Stone.

ΔΙΙΜЄΓΙϹΤѠΚЄΡΑΥ
ΝΙѠΥΠЄΡϹѠΤΗΡΙ
ΑϹ ΤΡΑ· ΑΔΡΙΑΝΟΥϹЄΒ
ΤΟΥΚΥΡΙΟΥΑΓΑΘΑΝΓЄ
ΛΟϹΑΒΙΛΗΝΟϹ ΤΗϹΔЄΚΑ
ΠΟΛЄΟϹ ΤΗΝΚΑΜΑΡΑΝѠΚΟ
ΔΟΜΗϹЄΝΚΑΙ ΤΗΝΚΛΙΝΗ
ЄΞ ΙΔΙѠΝΑΝЄΘΗΚЄΝ
ЄΤΟΥϹ ЄΜΥ ΜΗΝΟϹ ΛѠΟΥ

Hence it appears, that fourteen of the Palmyrene *letters have been handed down to us by this curious Inscription.*



Apud Joannem Swinton. S.T.B. Oxonienf. R.S.S.

ſcription with all the attention I was capable of, and took the tranſcript exhibited here upon the ſpot. In order therefore to rectify all former miſtakes, occaſioned by Sig. Pietro della Valle's blunders, I judged it might not be improper to communicate this, together with a new Latin and Engliſh verſion of it, attended by a copy of the correſpondent Greek inſcription, as it appears on the ſtone, to the Royal Society; ſubmitting to the conſideration of that illuſtrious body, with all poſſible deference, the ſhort remarks tranſmitted you in this paper.

For the Palmyrene inſciption, ſee TAB. I.

The inſcription in Hebrew or Chaldee Characters.

לבעל שמין מרא עלמא קרב
כסתא וערשא אגתגלס

JOVI FVLMINATORI IN AETERNVM ſit REVERENTIA—OPERIMENTVM ET LECTVM ei DEDICAVIT AGATHANGELVS.

TO JUPITER THE THUNDERER FOR EVER be REVERENCE—AGATHANGELVS DEDICATED to him this COVERED BED.

For the correſpondent Greek inſcription, as it appears on the ſtone, ſee TAB. I.

Remarks on the Palmyrene Inſcription.

1. That Baal, the great divinity of Syria and Phœnicia, anſwered to the ΖΕΥΣ of the Greeks, and the JUPITER of the Latins, as we find intimated by the two inſcriptions before me, is acknowledged by ſome

ſome of the moſt celebrated ancient writers; and has been clearly evinced by (2) me, in a former paper.

2. The word שמץ, SHEMETZ, ſometimes denotes NOISE, or SOUND, according to (3) Schindler, Schmidius, and Cocceius. It is likewiſe taken in Scripture for A VOICE IN THE AIR (4), as we learn from Stockius. BAAL SHEMETZ, therefore, may be rendered DOMINVS MVRMVRIS SONI, SVSVRRI, vel VOCIS IN AERE, THE LORD OF the NOISE, OF THE SOUND, or OF THE VOICE IN THE AIR; and conſequently may be deemed perfectly equivalent to ΖΕΥΣ ΚΕΡΑΥΝΙΟΣ, or JUPITER THE THUNDERER, in the correſpondent Greek inſcription. A new figure of *Tzade* here preſents itſelf to our view, which hat not been handed down to us by any other momument of the Palmyrenes.

3. The following word, מרא, or מורא, TIMOR, REVERENTIA, VENERATIO, *&c.* appears, in the very ſame ſignification, (5) in two Palmyrene inſcriptions, ſome years ſince by me explained. Nothing can be more common in the oriental languages than the ellipſis, or ſuppreſſion, of the verb (6) ſubſtantive, which occurs here.

(2) *Philoſ. Tranſ.* Vol. XLVIII. Par. II. p. 748, 749.

(3) Vid. Schindlerum Schmidium, Cocceium, Jo. Chriſt. Clod. *Lex Hebraic. Select.* p. 509, 510. Lipſiæ, 1744. & Jo. Leonhard. Reckenberger. *Lib. Radic. ſive Lex. Hebraic.* p. 1515. Jenæ, 1749.

(4) Chriſt. Stock. *Clav. Ling. Sanct. Vet. Teſt.* p. 1115. Jenæ, 1721. It muſt be remarked, that the word SHEMETZ, as explained here, will greatly illuſtrate two paſſages in the book of Job, (IV. 12. XXXVI. 14.) and decide in favour of the explication of thoſe paſſages given by Schmidius and Cocceius.

(5) *Philoſ. Tranſ.* ubi ſup. p. 698.

(6) Johan. Buxtorf. *Theſaur. Grammat.* p. 472. Baſileæ, 1663.

4. With

4. With regard to עלמא, of the Chaldee or Syriac form, it will be ſufficient to obſerve, that it anſwers here to AETERNITAS, PERPETVITAS, &c. So that מורא עלמא may be rendered REVERENTIA AETERNITATIS, or REVERENTIA AETERNA. The term עלמא may likewiſe be conſidered as equivalent to לעלמא, IN AETERNVM, the particle ל being not improbably here underſtood. Such ellipſes as this were by no means uncommon in the eaſtern world, as we find clearly evinced by (7) Noldius.

5. Nothing farther is requiſite to be obſerved of the Hebrew, Chaldee, or Syriac verb קרב (8), OBTVLIT, LIBAVIT, DEDICAVIT, &c. than that it ſeems perfectly conſonant to the tenor of the inſcription, and conveys to us the ſame idea that is exhibited to our view by the word ΑΝΕΘΗΚΕΝ, in the correſpondent Greek inſcription.

6. The next word כסתא, or כסותא (9), OPERIMENTVM, is intirely Syriac. It may not be improper to remark, that the letter *Thau* here is of a ſomewhat unuſual form; and that the *Vau* is underſtood, or ſuppreſſed, after the Phœnician manner. The figure likewiſe of the *Samech*, unleſs part of it has been effaced by the injuries of time, does not perfectly agree with any of thoſe characters that have been hitherto conſidered as forms of that element.

7. The copulative *Vau*, that follows, and connects כסתא with the ſubſtantive ערשא, or ערסא, LECTVS,

(7) Chriſtian. Nold. *Concordant. Particular. Ebræo-Chaldaicar.* p. 416, 417. Jenæ, 1734.

(8) Vid. Johan. Buxtorf. Sen. et Jun. Val. Schindl. alioſq. Lexicograph. Hebr. Chal. Syr. &c.

(9) Johan. Buxtorf. Jun. *Lex. Chald. & Syriac.* p. 260. Baſileæ, 1622.

SPONDA

SPONDA LECTI, &c. which is both a Chaldee and a Syriac word, renders the latter part of the inſcription ſufficiently intelligible. For OPERIMENTVM ET LECTVM here may be conſidered as equivalent to LECTVM OPERTVM, or perhaps ſimply LECTVM, as the correſpondent Greek word has been tranſlated by Dr. Bernard. The character repreſenting *Vau*, prefixed to the word I am now upon, agrees with the figure of that element, as it occurs in my ſecond Palmyrene (10) alphabet. For a farther account of theſe beds of ſtate, as well as ſeveral other ſimilar inſcriptions, recourſe may be had to the (11) author here referred to.

8. The laſt word of the inſcription is apparently אגתגלם, ΑΓΑΘΑΝΓΕΛΟΣ, AGATHANGELVS, the name of an Abilenian, who erected a cupola, or camera, and placed under it a bed of ſtate, dedicated to Jupiter Maximus Fulminator, or the ſupreme God Jupiter the Thunderer, for the health and ſafety of the emperor Hadrian, his ſovereign. This happened, according to the correſpondent Greek inſcription, in the 445th year of the æra of Seleucus, and the 17th of that prince's reign. The word ΑΓΑΘΑΝΓΕΛΟΣ, AGATHANGELVS, ſeems to be of nearly the ſame import with ΑΓΑΘΟΔΑΙΜΩΝ, AGATHODAEMON, the name of an (12) Alexandrian geographer of pretty conſiderable note. It alſo occurs in (13) one of Gruter's inſcriptions, but is evidently a cognomen there. The

(10) *Philoſ. Tranſact.* Vol. XLVIII. Par. II. p. 740.

(11) Seller. *Antiquit. of Palmyr.* p. 364, 369.

(12) Agathodæm. Alexandrin. per Mercator. et Bert. Amſt. 1618. Vid. etiam Agathodæm. Alexandrin. *Delineat. Orb. ex Lib. Ptolemæi*, Lat. Baſ. 1552.

(13) Jan. Gruter. *Inſcript. Romanar. Corp.* p. 644. 1.

character

character reprefenting *Ghimel*, in this word, is fomewhat different from all the other figures of the fame letter, that have hitherto appeared.

9. The Palmyrene alphabet deducible from this infcription [fee Tab. I.] being curious, as the forms of feveral of it's letters cannot be deemed the fame with thofe of the correfpondent elements in any of the Palmyrene alphabets hitherto publifhed; I fhould be thought guilty of an omiffion, did I not take the liberty to infert it here. The characters reprefenting *Ghimel*, *Samech*, *Tzade*, and *Thau*, in particular, differ confiderably from the figures of thofe letters on every other monument of the Palmyrenes.

As I have fpoken pretty largely of this infcription in a former paper, I fhall not expatiate any farther upon it here; but at prefent only beg leave to affure you that I am, with all due fentiments of refpect and efteem,

Sir,

Your moft faithful,

and moft obedient,

humble fervant,

Chrift-Church, Oxon.
Nov. 28, 1765.

John Swinton.

Received December 3, 1765.

III. *A Letter to* William Heberden, *M. D. Fellow of the Royal College of Phyſicians in* London, *and of the Royal Society, from* Daniel Peter Layard, *M. D. Phyſician to her Royal Highneſs the Princeſs Dowager of* Wales, *Member of the Royal College of Phyſicians in* London, *and of the Royal Societies of* London *and* Gottingen; *giving an Account of the* Somerſham *Water, in the County of* Huntingdon; *and tranſmitting a Letter from* Michael Morris, *M. D. F. R. S. Member of the Royal College of Phyſicians in* London, *and Phyſician to the* Weſtminſter *Hoſpital, to Dr.* Layard, *on the ſame Subject.*

Dear Sir,

Read Feb. 6, 1766.

AT laſt I venture to lay before you the reſult of thoſe experiments and obſervations, which I have made on the Somerſham water. They were undertaken with your approbation, and purſued through your encouragement.

To aſcertain the contents of a mineral water, requires a repetition of the analyſis; and notwithſtanding the experiments have been repeated theſe fourteen years laſt paſt, either at the ſpring, at Hunting-

 don,

don, or in London, and the effects of this water carefully obſerved in the many caſes it has been drank, yet I could not before preſume to offer you a poſitive determination, till I was convinced, by the trials of an abler and more competent judge, that I was not miſtaken. But now through the obliging and friendly aſſiſtance of Dr. Morris, I am enabled to communicate to you, and the public, the great utility and excellence of a medicinal water, well known and eſteemed many years ago. By a repetition alſo of every experiment made in the country, Dr. Morris, has in his laboratory, and in my preſence, aſcertained the contents and properties of the Somerſham water; nay more, as you will obſerve, Sir, by Dr. Morris's letter annexed, one conſiderable ingredient, namely allum, has been not only proved to exiſt in this water, but likewiſe the Dr. has produced cryſtals of allum, which Doctors * Lucas and Rutty † declare, have not yet been procured from any water, although allowed to contain an aluminous ſalt.

I ſhall not trouble you, Sir, with a detail of all the common experiments ſo well known to you, and which I had the ſatisfaction of ſhewing you, and many gentlemen of the different branches in the profeſſion of phyſic, at my houſe the two laſt ſummers; but proceed to an account of the water, and it's contents.

The Somerſham water, commonly called the Somerſham Spa, iſſues out from the declivity of a ſmall hill, which is ſituated on a heath, bearing the ſame

* Vol. II. p. 24. Eſſay on Waters.

† P. 299. Synopſis of Mineral Waters.

name, and laying near the high road, between the towns of St. Ives and Somersham, in the county of Huntingdon, about three miles distance from St. Ives. This heath was formerly covered by part of the royal forests cut down in the reigns of Henry II. III. or of Edward I. and now serves only for the grazing of sheep.

The different strata of the earth on this heath are, immediately under the swerd, first, about six inches depth of mould, or arable, then different strata of clay, each stratum about ten or twelve inches deep, growing darker from a yellowish or grey color, to a dark blue golt the deeper it lies. At about seven feet deep is found a bed of gravel, out of which the water springs forth very clear. This stratum of gravel is about twelve or fourteen inches thick, surrounded with a bed of a very dark blue golt, beset with large quantities of selenites, which have shot in it, and are surrounded with some fine yellow clay sticking to them.

The water, flowing from this spring,, which is perennial, but runs more or less according to the different seasons, is received from the bed of gravel by three small brick channels, about two feet long, which meet in one of about six inches square. This channel which is near twenty feet in length, conveys the water to a bason also of brick, and about two feet square. The channel and bason were made about 40 years ago under the direction of the late Rev. Dr. Knight; were opened and cleaned in the years 1755 and 1759, when I directed the workmen to new lay the bricks of the channel in some of the stiff blue clay, instead of lime-mortar, that the water might be

be leſs liable to a decompoſition in its paſſage. From the baſon the water frequently overflows, and runs trickling along a trench into a ſmall brook; where-ever it ſtops, it leaves the clay ground tinged of a ruſty ochry color, and the water ſtagnating is covered with a thick pellicle variegated of many colors. In froſty weather, I have often found ſtalactites hanging round the edges of the baſon, which, upon examination, were formed by the ſelenites impregnated with the vitriol of iron, changing their white appearance to a ruſty yellow-colored cruſt covering the ſtalactites.

The water taken up out of the baſon, is generally clear and tranſparent, unleſs after heavy rains, when it appears thick and of a muddy yellow; or when the baſon has not been cleaned ſome time, lumps of a black gelatinous ſubſtance, like the ſediments in ink bottles, are taken out from the bottom of the baſon.

By dipping carefully a glaſs into the baſon in a dry ſeaſon and fair weather, the water is quite clear, full of bubbles ſparkling up, ſome of which ſtick to the ſides of the glaſs. By the hydroſtatical ballance, its weight differs from diſtilled rain water, weighed at the well, as 1006 $\frac{1}{2}$ is to 1000; when carried to Huntingdon, as 1010, to 1000; and brought to London, it has about the ſame weight.

The Somerſham water, drank at the ſpring, is cool, pungent, and of an auſtere, ſharp, aſtringent, ferruginous taſte, ſomewhat inky, but not in the leaſt diſagreeable; when carried to any diſtance, it loſes a little of its pungency, by its ſuffering a decompoſition; but carefully bottled under water, and

then well corked, covering afterwatds the corks closely with rosin and wax, the water preserves its briskness and volatility a long while, and provided the bottle be kept corked, though half the water may have been drawn out, yet after keeping it months, nay many years, it will still preserve its irony principle, so as to turn with galls, purple, or dark blue.

Experiments at the Spring Head.

Experiment I.

Half a grain of powdered galls turns a pint of Somersham water of a dark purple.

Experiment II.

Brandy, or rum, drawn from oak casks, or an infusion of green tea leaves, turns it of a blackish cast.

Experiment III.

Vegetable, nor mineral acids, cause the least alteration in the water, which remains clear, and without the least effervescence.

Experiment IV.

Alcalies, whether volatile or fixed, cause an effervescence and turn the water green and curdled.

Experiment V.

It mixes with milk without alteration.

Experiment VI.

Syrup of violets turns the water green.

Experiments at home, in Huntingdon.

Experiment VII.

Half a grain of galls powdered turned a pint of Somerſham water of a dark mazarine blue, inclining to purple.

Experiment VIII.

In two vials were poured ſome Somerſham water, each vial containing eight ounces. In one had been put two drams of filings of iron. After ſtanding twenty-four hours in a moderate warm place, three grains of powdered galls were added to each vial. The pure Somerſham water immediately turned purple, with the galls, which next day fell like a purple ſediment to the bottom, and left the ſupernatant liquor clear. The water poured on the filings of iron threw up ſeveral air bubbles, turned black as ink, ſome few hours after the addition of the three grains of galls; and kept that color, and an inky taſte, ſeveral days; after which the black ſediment fell likewiſe to the bottom, and left a clear ſupernatant water, which ſtill preſerved its inky taſte.

Experiment IX.

Brandy, or rum, drawn from oak caſks, or an infuſion of green tea leaves, produced a purple color, inclining to black.

Experiment X.

Somerſham water doth not lather with ſoap, but curdles immediately.

Experiment XI.

Acids cauſe no alteration, but mix as at the ſpring head.

Experiment XII.

Volatile or fixed alcalies did not effervesce with the Somerſham water, at home, as at the ſpring head, but produced a cloud, then curdled, and threw down a ſediment.

Experiment XIII.

The Somerſham water, in a few hours, lets fall an ochry ſediment to the bottom of the bottle, which is of a different color, according to the ſeaſon in which the water is taken up; in dry times, the ſediment is of a ruſty, cinnamon, or orange color; in wet weather, it is of an olive, or brown color, ſmelling like the fumes of ſulphur.

Experiment XIV.

After the firſt ſeparation of the ochry ſediment, the water continues clear, and tranſparent; and notwithſtanding I have kept ſome bottles half full, months, and years, yet the water preſerved its purity, and would in a leſſer degree turn purple with galls.

Experiment XV.

The water being filtred from the ochre, which was precipitated at the bottom of the bottle, and then put on the fire, emits many air bubbles, then grows turbid and yellow, and after a little evaporation throws up a thick ſcum, which ſeparated by filtration proves to be a fine ochre beſet with ſelenites. The water will then, after boiling, turn purple with galls. By accident I let fall ſome hot Somerſham water on an iron fender, which it inſtantly turned to ſo many blue ſpots, as there were drops on the fender.

Experiment XVI.

After this ſecond filtration, the water appears greeniſh at the top, and lets fall a white ſediment; when the liquor is evaporated to nearly the quantity of half an ounce, a thick pellicle is formed.

Experiment XVII.

If to this reſiduum a ſmall quantity of diſtilled rain water be added warm, and then filtred, a very

 white

white ſediment will be ſeparated; and the rain water being evaporated to a pellicle, will leave a brown depoſit, which will appear to be a bitter muriatic ſalt, containing ſometimes a few cryſtals.

Experiment XVIII.

The Somerſham water mixes well with warm and boiling milk, when carried at a diſtance as well as at the ſpring head; but when equal quantities of the water and milk are boiled together, then the milk is turned immediately, the curd of which becomes of a bright pink color with galls, the whey of a reddiſh brown, and both are of an agreeable rough and ferruginous taſte.

Experiment XIX.

To diſcover if the water contained any allum, according to Dr. Shaw's direction, ſome leaves of the herb Geranium Robertianum, commonly called Ragged Robert, were *infuſed* in *four* ounces of Somerſham water, a compariſon made between this infuſion, and one of the ſame herb, in the ſame quantity of diſtilled rain water, and another in as much diſtilled rain water, with two grains of allum powdered. The glaſſes were all placed in a moderate heat, and the ſeveral infuſions appeared, as follows, after ſtanding twenty four hours; the Somerſham water of a purple color, with a reddiſh hue; the rain water, with allum, of a green, with a reddiſh caſt; and the rain water of a fine green. The herb ſubſides with the Somerſham water, floats

in the middle with the aluminous rain water, and ſwims on the top in the ſimple rain water. The herb is turned purple, by infuſion, in the Somerſham water; is of a yellow green in the rain water with allum; and remains of a bright green when infuſed in rain water alone.

Experiment XX.

The ſediments, by evaporation, from Somerſham water differ, as is ſaid, of the ſpontaneous ſeparation, in Experiment 13. according to the ſeaſons, both as to color and quantity. Four pounds of the Somerſham water have, by evaporation, yielded in a dry ſeaſon, ſixteen, or at moſt twenty grains of a ruſty colored, or orange colored ſediment; whereas, after rainy weather, two pounds of the ſame water have yielded one dram of a dark olive brown ſediment.

Experiment XXI.

I put two drams of the yellow ochry ſediment into a crucible, covered with a tile, and having calcined it in an open fire, it had loſt a ſixth part of its weight; the reſiduum appeared of a red ruſty color, mixed with a white earth; and on the ſides of the crucible a ſmall quantity of greyiſh powder ſticking to it. The red powder was partly attracted by the magnet.

Experiment XXII.

I put, at the ſame time, into another crucible, a lump of the blue clay, brought from the Somerſham Spa,

weighing about two ounces; this clay was intermixed with ſmall ſhoots of ſelenites, ſurrounded with the fine yellow ochry clay mentioned above. After calcination, part of the clay had vitrified, part was become of a dark red, and the ſelenites were burnt to a white powder.

Experiment XXIII.

I threw ſome of the white ſediment, procured by filtration and evaporation mentioned in Experiment 17. on a red hot iron; it partly bliſtered up, and turned of a greyiſh color, and the remainder, which was the greater quantity, appeared of a very clear white.

Experiment XXIV.

Having, by evaporation and filtration, procured ſome ſelenites from the Somerſham water, I threw it into a red hot crucible, wherein it partly ſwelled up like bliſters, and when cold appeared of a greyiſh white, intermixed with particles of iron.

Theſe Experiments plainly demonſtrate, Sir, that the following contents are to be found in the Somerſham water. Firſt, by Exp. 1, 2, 6, 7, 8, 9, 13, 14, 15, 18, 20, 21, 24, iron. Secondly, by Exp. 13, 16, diſſolved pyrites. Thirdly, by Exp. 1, 3, 4, 10, 11, 12, 14, 16, 18, a vitriolic acid. Fourthly, by Exp. 22, 23, 24, a calcarious earth. Fifthly, by Exp. 15, an ochre. Sixthly, by Exp. 15, 16, 21, 22, 23, 24, ſelenites. Seventhly, by Exp. 17, a muriatic ſalt, which doth not cryſtallize, And, Eighthly, by Exp. 18, 19, 21, 23, 24, allum.

The

The Somersham water, therefore, is a chalybeate water, strongly impregnated with the vitriol of iron and allum, and containing some calcarious earth, selenites and salt.

It would be absurd in me to trouble you, Sir, either with any more experiments, or the relation of the several cases, in which I have found the salutary effects of the Somersham water, since no one knows its properties better than you, Sir, nor directed it formerly with greater efficacy. I shall, therefore, now lay before you Dr. Morris's letter, in confirmation of what I have said, and only mention, that upon the several trials made with this water on human calculi, both at Huntingdon and in London by me, and lately by Dr. Morris, the Somersham water has acted powerfully on those substances; that it mixes well with the blood, and passes visibly with the urine. But these experiments, and the above-mentioned cases, trite as they must be to you, may probably, with other particulars relating to the county of Huntingdon, be laid before the public at some other time.

I am happy in this opportunity of acknowleging myself, with the greatest regard,

Dear Sir,

Your most obliged,

and most obedient humble servant,

Lower Brook-street,
Nov. 15, 1765.

D. P. Layard.

A Letter from Michael Morris, *M. D. Member of the Royal College of Physicians in* London, *and of the Royal Society, to* Daniel Peter Layard, *M. D. Physician to Her Royal Highness the Princess Dowager of* Wales, *Member of the Royal College of Physicians in* London, *and of the Royal Societies of* London *and* Gottingen, *relating to Experiments made on the* Somersham *Water.*

Dear Sir,

FROM your very accurate account of the singular mineral water, found at Somersham, in Huntingdonshire, and its salutary effects in many obstinate diseases, it appears to be highly deserving the attention of the public. I, therefore, agreed with pleasure to your proposal of repeating the experimenrs here, which you had formerly made at the spring head, and at Huntingdon; and adding such as you were obliged to omit in the country for want of a proper apparatus.

As you intend to oblige the public with an account of the former, to which our repeated trials here were perfectly conformable, I shall confine myself in the following narrative, principally to the latter, at which you assisted in my laboratory.

Experiments on the Somersham Water.

The water seemed clear in the bottles, though there was some sediment at the bottom. It was clear and bright when poured into a glass, but did not sparkle nor emit air bubbles for a considerable time

time. It was auſtere, ſubacid, and chalybeate to the taſte.

Experiment I.

On adding five grains of powdered galls to a glaſs of the water, it ſoon became of a muddy blue, which in a little time changed to a light purple.

Several bottles of the water, which had been kept upwards of two months, exhibited the ſame appearances.

The water, in ſome bottles, which had been half emptied on purpoſe, and corked ſlightly, ſtill preſerved its property of ſtriking a blue and purple with galls, though more faint.

From theſe experiments, the Somerſham water appears manifeſtly to have preſerved its ferruginous quality, notwithſtanding the long carriage from the ſpring to London, and to have loſt very little of it for a conſiderable time after, which renders it a valuable acquiſition to the public, as there are very few mineral waters, generally known in England, that do not loſe their chalybeate properties in a few days, and even at a ſmall diſtance from their ſource; which renders the importation of the foreign chalybeate waters abſolutely neceſſary, at a conſiderable price.

Experiment II.

Two pounds of the Somerſham water were expoſed to a gradual fire in a glaſs retort luted to a receiver.

The water, as it grew warm, became turbid, and diſcharged air bubbles, depoſiting at the ſame time an ochry ſediment at the bottom. Theſe appearances ceaſed before eight ounces were drawn off by diſtillation.

The diſtilled water, proving on examination, to be no wiſe different from common diſtilled water, the diſtillation was diſcontinued; the contents of the retort were poured into a white baſon; on ſtanding a night to ſettle, the liquor in the baſon ſeemed clear, and a yellow ſediment was ſeen at the bottom; the clear liquor was decanted into another baſon.

The ſediment carefully dried, weighed near four grains, and proved to be chiefly ferruginous.

The clear liquor was ſuffered to evaporate, by the heat of the atmoſphere, in the month of Auguſt; pellicles were formed ſucceſſively on the ſurface, which breaking in a little time fell to the bottom; this continued until there remained about two ounces of liquor, which was poured carefully from the pellicles into a cup, and was ſet to evaporate in a moderate heat. The dried pellicles weighed 30 grains. They were inſipid, gritty, and not ſoluble in water.

Experiment III.

Six grains of the pellicles, expoſed to a ſtrong fire, in a covered crucible, for three hours, became reddiſh when cold, and loſt a grain in weight; but ſeemed not altered in other reſpects.

Experiment IV.

Six grains of the ſame pellicles expoſed on a teſt to a reverberatory heat for the ſame ſpace of time; the vitriolic acid being volatilized, by the reverberated flame, was expelled from its terrene baſis, ſo that the reſiduum, when cold, weighed but three grains, was acrid to the taſte, grew hot with water, and communicated the ſame qualities to it that lime does. Hence the ſaline pellicles depoſited by evaporation, appear to be the ſelenites, or the vitriolic acid, united to a calcarious earth, with a little iron.

Experiment V.

The liquor in the cup being quite evaporated, there remained ſome regular cryſtals ſtanding in a whitiſh powder; the cryſtals weighed five grains, and proved on examination to be regular cryſtals of allum.

Experiment VI.

The whitiſh powder ſoon attracted moiſture from the air, and in the ſpace of 24 hours ran, *per deliquium*, into a browniſh ſubacid auſtere liquor, of a ferruginous and ſaline taſte.

It appears from theſe experiments, that the contents of the Somerſham water are: Firſt, Iron. Secondly, Selenite. Thirdly, Allum. Fourthly, From its taſte, and attracting the moiſture of the air, ſome marine ſalt, with a little allum and vitriol

in the ſtate of an *aqua magiſtra aluminis & vitrioli*, incapable of cryſtallization.

The Somerſham water, therefore, ſeems to differ conſiderably from any of the mineral waters known in Great Britain or Ireland *. For among the writers, who even admit and mention aluminous waters, no one, except Dr. Short, pretends to have ſeparated cryſtals of allum from them; and even he declares, that in ſubſequent trials on the Nevil Holt water (from which he had once obtained ſome) he could not ſucceed.

Though if we conſider that allum is compoſed of the vitriolic acid, united to an argillaceous earth, it will not be difficult to conceive that an acid water, paſſing through a ſtratum of ſuch earth, ſhould act upon, and unite with a ſmall portion of it; or the water may diſſolve ſome allum in the ſtratum of decompoſed pyrites, where it is impregnated with iron and ſelenite. So that probably allum has been often overlooked in water wherein it exiſted; nor did we obtain any in our experiments, until nearly all the ſelenite had been ſeparated.

However, as allum is a very powerful medicine; the quantity diſcovered in the Somerſham water, muſt have contributed not a little to its efficacy, in ſome of the remarkable caſes wherein you have obſerved its ſucceſs.

I am,

SIR,

Your obliged humble Servant,

Michael Morris.

* Rutty Synopſis of Mineral Waters, p. 299. Lucas Eſſay on Waters, Vol. II. p. 24.

Received December 12, 1765.

IV. *Account of an inedited Coin of the Empreſs* Criſpina. *In a Letter to the Rev.* Thomas Birch, *D. D. Secretary to the Royal Society, from the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

S I R,

Read Feb. 13, 1766.

AN inedited Greek coin of the empreſs Criſpina, which ſeems to have had a place formerly aſſigned it in the cabinet of the celebrated Profeſſor Ott, and from thence afterwards to have paſſed into that of his ſon, the late Reverend Mr. Ott, ſome years ſince fell into my hands. The medal is nearly of the ſize of the middle Roman braſs, and tolerably well preſerved. The workmanſhip is ſomewhat rude, and ſavours ſufficiently both of the age and the remote province in which it firſt appeared. On one ſide is exhibited the head of Criſpina, wife of the emperor Commodus, attended by the Greek legend ΚΡΙϹΠΕΙΝΑ ϹΕΒΑϹΤΗ, CRISPINA AVGVSTA; and on the reverſe two human figures, one ſitting in a chair, with a lance in its left hand, and the other ſtanding by its ſide, preſent themſelves to our view. They are both

 ſurrounded

furrounded by the infcription ΔΑΡΔΑΝΟCCHNΩΝ, DARDANOSSENORVM, or DARDANOSSENSIVM, which evidently points at the inhabitants of fome antient town. Not one of the letters, either of the legend or infcription, has fuffered greatly from the injuries of time.

Who the Dardanoffenians were, or in what part of the world fituated, I muft not take upon me abfolutely to decide; the word ΔΑΡΔΑΝΟΣΣΑ, DARDANOSSA, not appearing, as the name of a city, in any antient writer. But that this word occurred, in fuch a fenfe, in the original text (1) of Ptolemy, and was afterwards converted by fome ignorant tranfcriber into DARANISSA, which ftill remains in all the printed and manufcript copies of that author, will, I perfuade myfelf, not be contefted by the critics of the prefent age. The coin therefore was ftruck at Dardanoffa, or Daraniffa, which feems to have been a town feated in Sophene, a province of the Greater Armenia, in the reign of the emperor Commodus, where the Roman power at that time prevailed. And this is confonant to the faith of hiftory (2), from whence we learn, that the conqueft of Armenia was effected, after the reduction of Artaxata, by Statius Prifcus, not many years before Commodus afcended the imperial throne. Nay, the whole country is faid to have been conquered, and reduced to the form of a Roman province, in the days of Trajan. The figure of the *Sigma* likewife on this medal, fo fimilar to the form of that element on certain Armenian

(1) Ptol. *Geograph.* Lib. V. c. 13.
(2) Dio, Lib. LXVIII. Jul. Capitolin. in *Marc.* & in *Ver.* Lucian. p. 347. Jamblichus apud Photium, p. 242.

coins

coins of the ſame age, will bring a freſh acceſſion of ſtrength to the notion here propoſed to the conſideration of the learned; if it will not, in conjunction with what has been offered, evince this beyond the poſſibility of a doubt.

As the medal before me has never been hitherto publiſhed, nor perhaps ever ſeen in any other cabinet, either of the curious or the learned, I was inclined to believe, that a deſcription of it, as well as the draught of it attending this paper, [See TAB. I.] might not prove altogether unacceptable to the Royal Society; eſpecially, as it enables us to emend the corrupted proper name of a town in Ptolemy, and evinces Dardanoſſa, or Daraniſſa, to have been ſubject to Commodus when he preſided over the Roman world. Nor can any thing, as the authority of MSS. muſt give way to that of antient coins and inſcriptions, be better ſupported than ſuch an emendation. Nor have any coins of this city been ever diſcovered before. All which will, I flatter myſelf, be deemed a ſufficient apology for the trouble given on this occaſion by,

S I R,

Your moſt obedient humble Servant,

Chriſt-Church, Oxon.
Dec. 7, 1765.

John Swinton.

V. *Obſervations*

Received December 5, 1765.

V. *Observation of the Eclipse of the Sun, of* August 16, 1765, *made at* Leyden, *by Professor* Lulofs, *F. R. S. to* Charles Morton, *M. D. Sec. R. S.*

Carolo Morton, M. D. Soc. Reg. Lond. Secretario, & Academiar. Imperial. Leopoldin. & St. Petropolitan. Socio. Joannes Lulofs, Academ. Leidens. Matheseos Professor. Societ. Reg. Lond. Socius, *&c.* S. P. D.

Read Feb. 13, 1765.

ABbas de la Caille (*Ephemerides des Mouvements Celestes pour l'année* 1765) initium deliquii pro urbe Amstelædamensi determinaverat $5^h\ 4'$ post merid. finem $5^h\ 24'$, quantitatem o dig. 5'. Cum de la Caille conspirabat peritissimus de la Lande, maximamque eclipseos phasin locum habituram prædixerat in eadem urbe $5^h\ 15'$.

Hisce determinationibus plus fidens, quam rudioribus, quas ipse tantum feceram, ut typum deliquii in usum observatorii nostri parare mihi liceret, (secundum quas initium esset Leydæ $4^h\ 10'$, finis $5^h\ 10'$, quantitas $1^d\ 22'$) aliisque insuper negotiis distractus, observatorium conscendi hora 4, sed, disposito telescopio Newtoniano septem pedum, eidemque adplicato micrometro Bradleyano, admirabundus percepi, hora 4 29′ 1″ eo-usque jam proventam fuisse Lunam,

Lunam, ut diſtantia cornuum deliquii 19′ 36″ adæquaret; unde quantitas obſcurationis jam erat 2 dig. & 38′; ab eo tempore non admodum increvit eclipſis, ita ut non ultra 2 dig. & 40′ vel 42′ adſcenderit. Hora autem 4 44′ 48″ diſtantiam cornuum denuo deprehendi 19′ 36″, adeoque medium deliquii non longe abſuit ab hora 4 36′ 55″.

Reliquæ obſervationes circa diſtantiam cornuum, & magnitudinum obſcurationis exinde erutarum determinationes, ſunt ſequentes.

Temp. ver.			diſtantia cornuum		Magnitudo.	
	′	″	′	″	dig.	′
4^h	51	45	18	51	2	24
	55	55	18	6	2	$11\frac{1}{2}$
	59	19	17	21	2	1
5	3	51	15	50	1	38
	7	9	14	20	1	20
	12	42	11	18	0	48
	17	21	6	2	0	$13\frac{1}{2}$
	18	58 Finis				

Datum Lugd. Batav. d. 5 Novemb. 1765.

VI. *A Letter from* James Parſons, *M. D. F. R. S. to the Right Honourable the Earl of* Morton, *Preſident of the Royal Society; on the the double Horns of the Rhinoceros.*

My Lord;

Read Feb. 27, 1766.

WHEN I had the honor of laying my natural hiſtory of the Rhinoceros before this learned Society in 1743, which is printed in number 470, page 523, of the Tranſactions, I had not an opportunity of ſhewing a double horn to the members; I have, therefore, taken this firſt occaſion to entertain the preſent members with a ſight of a noble ſpecimen of the horns of an African Rhinoceros, brought from the Cape of Good Hope, by my curious and worthy friend William Maguire eſquire, among many other curioſities; preſuming that few of the Society have ever ſeen a pair of the like kind. But what renders this ſubject the more particular, and worthy of obſervation, is that, by means of knowing there is a ſpecies of this animal, having always a double horn upon the noſe, in Africa, Martial's reading is ſupported againſt the criticiſm of Bochart, who changed the true text of that poet, in an epigram upon the ſtrength of this animal; for when Domitian ordered an exhibition of wild beaſts, as it was the cuſtom of

of ſeveral emperors. The poet ſays: The Rhinoceros toſs'd up a heavy bear with his double horn:

Namque gravem gemino cornu ſic extulit urſum.

and as Bochart knew nothing of a double horn, he changed this line both in reading and ſenſe, thus:

Namque gravi geminum cornu ſic extulit eurum.

as if two wild bulls were toſſed up into the air, by the ſtrong horn of the Rhinoceros.

Mr. Maittaire adopted the notion of a ſingle horn, but was of opinion that the *geminum eurum* of Bochart ought to have been plural, *geminos euros*, as being more elegant; and he was followed by Doctors Mead and Douglas, with this difference, that theſe changed the *euros* for *urſos*, as imagining they were rather bears than bulls, that were thrown up by this noble animal.

Our then worthy preſident Martin Folkes Eſquire, had ſeen my account of this ſubject, at the end of which, I endeavoured, however preſumptuouſly, to defend Martial's reading againſt Bochart and the other eminent perſons mentioned; and deſired I would let it be read and printed, which I very readily agreed to, as his requeſt did me much honour.

Before my paper was printed, Mr. Maittaire and Doctor Douglas died; and the learned Doctor Mead was the ſurviving critic, upon this line, of the three. Upon this occaſion, therefore, I have a double pleaſure; firſt, in amuſing the preſent gentlemen with a moſt curious ſpecimen in natural hiſtory; and, ſecondly, in remembring, in this place, the nice candor and generoſity of Doctor Mead upon that ſubject. For, about four months after the paper was printed, he received a preſent of ſeveral cu-

rious ſhells, ſeeds, &c. and with them the bones of the face of a young Rhinoceros, with two horns *in ſitu*, all intire, by a captain, of an African trader, who brought them from *Angola*.

As ſoon as he ſaw the horns, he ſent to invite me to breakfaſt, and there, in company, ingenuouſly gave up his paſt opinion, and declared for Martial; and, indeed, I muſt add to the praiſe of that great man, that, as I was happy in being frequently at his houſe, I was witneſs to many ſuch inſtances of the moſt diſintereſted candor and generoſity, where any part of ſcience was the topic, among his ſelect friends.

This anecdote I thought proper to mention upon the preſent occaſion; nor can too much be ſaid to his honour, among all lovers of philoſophical learning. I am,

Your Lordſhip's

moſt obedient Servant,

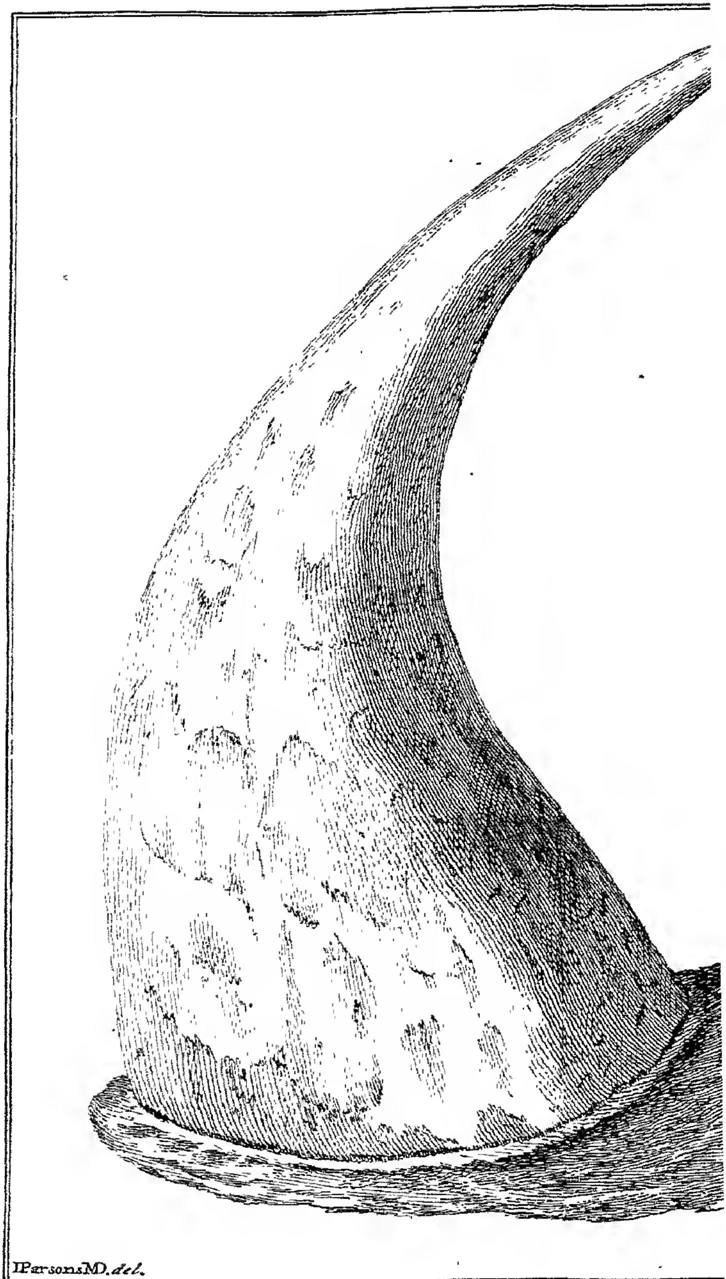
James Parſons.

P. S. The figure of the double horn of the Rhinoceros here deſcribed is ſeen in TAB. II. The dimenſions are as follows; viz. The length of the anterior horn, meaſuring with a ſtring along the convex fore part, is 20 inches; perpendicular height 18; circumference 21 $\frac{1}{2}$ at the baſe; the poſterior horn is in perpendicular height 19 $\frac{1}{4}$; circumference round the baſe 18; length of both baſes together upon the naſal bones 14; and the weight of both together is 14 pounds 10 ounces.

The Rhinoceros of the year 1739, deſcribed in the Tranſactions, was three years old; and the horn not three inches high; and hence by comparing that with this, one may imagine this to be many years old, perhaps above twenty; and that this animal lives to a great age.

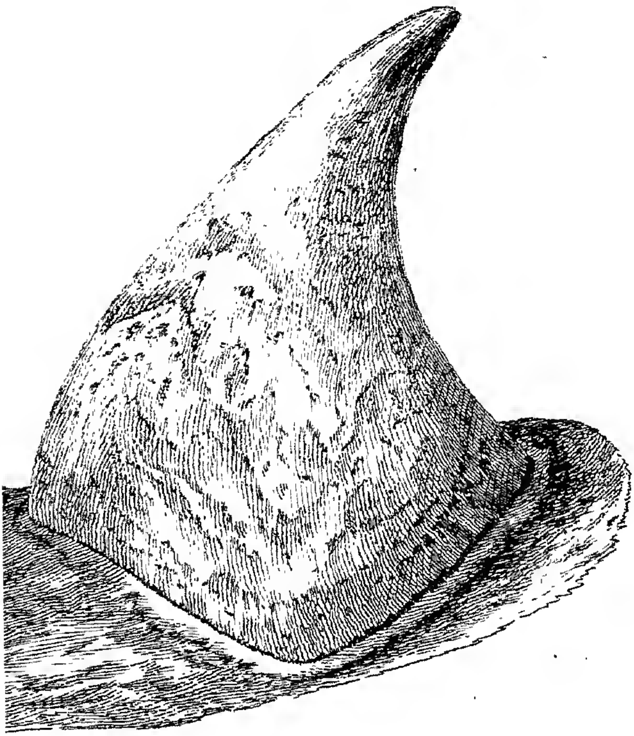
It is alſo plain that the horns are perpetual as are thoſe of oxen.

VII. *Extract*



I Parsons MD. del.

The Double Horn of an African Rhinoceros, brought from the Cape of Good Hope, by William M^c. Guire Esq^r.



J. Hynde sc.

Received February 20, 1766.

VII. *Extract from Two Letters, dated* December *7th and 12th,* 1765, *from the Rev. Mr.* William Borlase, *of* Ludgvan, *in* Cornwall, *F. R. S. to* Emanuel Mendes da Costa, *Librarian,* &c. *to the Royal Society.*

Dear Sir;

Read March 5, 1766.

ON board the Spackman tin ship, I have sent a contribution to the fossil collection of the Royal Society, which I beg you will present with my duty and respects to that illustrious body; it is

NATIVE TIN,

a rarity indeed! and I shall not be easy till I hear it is safely lodged in your Museum, as it is the fairest specimen I have seen.

As the existence of Native Tin is absolutely denied by all mineralists both antient and modern; and at the time I wrote my Natural History of Cornwall, having no evidences before me to evince the contrary, I contented myself with suggesting, page 185, that its existence was far from being improbable, and in that manner I left the dispute undecided. But a late fortunate discovery, which has furnished me with

three ſpecimens of this metal native, or pure, will now exclude all further doubt.

The account of it is as follows. In the month of May laſt was found near St. Auſtle, by ſome ſtreamers, a large cake, or nodule, of tin ore, weighing about ſix pounds, irregular in ſhape, cracked or jagged at the edges, lying about five feet under the ſurface, and in the middle of that ſtratum of tin ore, ſo remarkably ſpread in the moor adjoining to the forementioned town (*vide* Natural Hiſtory of Cornwall, page 163). When the lump was broke, it appeared to conſiſt of two coats, or incruſtations, ſurrounding the whole, and of a nucleus or central ſubſtance of a quartz intermixed with the pureſt malleable tin.

The firſt ſpecimen is now lodged in my deſk of Corniſh foſſils at the Muſeum in Oxford. The outmoſt cruſt was about $\frac{1}{8}$ of an inch thick at a medium, and of a browniſh ſtraw color; the ſecond or inner coat was blacker, cloſer grained, with ſome faint appearances of whitiſh ſpecks interſperſed, and about $\frac{1}{3}$ of an inch thick; theſe two coats incloſed a third ſubſtance, conſiſting of laminated cryſtals, riſing ſide by ſide out of an edging, which ſhines like melted tin, and lies as it were at their roots coherent to the ſecond coat. Theſe cryſtalline laminæ are thin almoſt as the flakes or ſcales of talc, and being ſhot in a great variety of directions interſect each other, and leave a vaſt number of cells, within which are plainly ſeen, and may be cut freely with a knife, many ſpecks and granules of pure native tin.

The ſecond ſpecimen, which I have the honor to preſent to the Muſeum of the Royal Society, is of

of the ſame ſtructure, and part of the above-deſcribed lump, but is much richer in quality. It was ſent me on the 12th of Auguſt laſt by Mr. Henry Roſewarne, of Truro, a gentleman well verſed in the knowledge and fuſion of metals. Beſides all the appearances of native tin taken notice of, in the former ſpecimen, here, in this N° 2. we ſee the malleable tin, in color equal to the fineſt tin of the furnace, more liberally and diſtinctly diſperſed. The metal is not only found in granules, but in a foliaceous manner iſſuing out of the quartz, and formed like a thick, jagged, or ſcolloped lace or edging, of which the ſpecimen itſelf only can give the juſteſt idea.

The lump, or nodule, of which theſe two ſpecimens are fragments, was ſo richly impregnated with tin, that though the beſt tin ore, in general, will not melt without flux, nor do twenty pounds of black tin uſually produce more than fourteen pounds of white, this melted without flux, and twenty ounces produced eighteen ounces of the pureſt tin.

The third ſpecimen is as follows. On the 17th of July laſt was found in a ſtream work near the borough of Granpont, and two days after brought to the above-mentioned Mr. Henry Roſewarne, by Jonathan Crowle, tinner, another lump of the ſame kind of tin ore as the former; it's weight between eleven and twelve pounds; the native tin was incloſed ſo ſecurely, that, but for the extraordinary weight, it had paſſed unnoticed. Within the cruſt, the metal was not in granules, as in the firſt ſpecimen, nor thin as a leaf, as in the ſecond; but much more abundant,

and in ſome places more than one inch thick; but unfortunately the perſon employed to diſcover the contents, injudiciouſly broke the outward cruſt all to pieces, by which means he got all the pure metal indeed, but prevented that abſolute conviction, which this noble ſpecimen would otherwiſe have conveyed to the doubtful. However, the like ſtructure of the cruſt, and that of the before-mentioned ſpecimens, and its connexion, to ſome fragments ſtill in the keeping of Mr. Roſewarne, with the granulated ſurface, and ſhotten edge, of the metal, pronounce it, upon compariſon, to be native tin.

The cruſt, incloſing this third ſpecimen, was certain ſtone of the quartz kind, very hard to break, and exactly the ſame, to all appearance, with that of the firſt maſs. I employed a tinner dextrous in vanning (a way of breaking and trying ores, by waſhing them on a ſhovel gently with water) to try it in his uſual way; he bruiſed it in my ſight, and obſerved to his ſurpriſe, that it ſuffered no diminution, or decreaſe, as all other ores do; that it was very rich in its kind; that he had never ſeen any ſuch before; and that he could not ſay what metal it contained.

Thus far is the relation Mr. Borlaſe gives; but as the exiſtence of native tin is ſo univerſally doubted, I thought it neceſſary, that other proofs than a meer hiſtorical account, and the exhibition of only two ſpecimens, and both from the ſame hand, ſhould be produced to prove it. Mineraliſts might then doubt whether what Mr. Borlaſe calls tin, was really that metal, or rather an arſenical marcaſite, or other mi-

 neral,

neral, which might appear like, or be miſtaken for, tin. I thought it very neceſſary to remove all doubts, by making proper experiments to try if it was tin, before I preſumed to communicate it to this learned body; it being ſo extraordinary a diſcovery. The experiments I made, and which, I hope, will prove ſatisfactory, to convince every one that it is really tin, are as follow,

1. It is perfectly ductile and malleable; and, bent between the teeth, gives the ſame crackling noiſe as tin always does.

2. In an open fire it melts eaſily, calcines on the ſurface, and ſmokes ſomewhat; forced in a ſtronger fire, with borax, it detonates with ſmall phoſphoreſcent ſparks, which is a property of pure tin.

3. It is only corroded to a white calx in ſpirit of nitre, and oil of tartar *per deliquium* being added to the ſolution, not any thing was precipitated.

It is, therefore, pure Tin.

I am, with great reſpect and obedience,

Gentlemen,

Your ever devoted,

and moſt obliged humble ſervant,

Feb. 20, 1766.

Emanuel Mendes da Coſta.

Received January 3, 1766.

VIII. *A Letter from* Edward Wortley Montagu, *Esquire, F. R. S. to* William Watfon, *M. D. F. R. S. containing an Account of his Journey from* Cairo, *in* Egypt, *to the Written Mountains, in the Defart of* Sinaï.

S I R,

Read March 13, 1766.

IT is with a good deal of difficulty that I have prevailed upon myfelf to write to you, for, as coming now to Italy was quite unforefeen, and I am immediately going back to the Eaft, I have not my journal with me, but luckily have the famous infcriptions. I am fenfible every paper I fend to the Royal Society expofes more and more my incapacity. However, as thefe infcriptions are much wanted, I cannot avoid fending them. I fhall only fpeak to fome of the points the bifhop of Clogher mentions; but cannot avoid being now and then a little prolix.

I fet out from Cairo, by the road known by the name of Tauriche Beni Ifrael, Road of the Children of Ifrael. After twenty hours travelling, at about three miles an hour, we paffed, by an opening in the mountains on our right hand, the mountains Maxattee. There are two more roads, one to the northward of this, which the Mecca pilgrims go, and one to the fouth,

ſouth, between the mountains, but never travelled (as it does not lead to Suez, to which it is thirty hours march from Cairo). Through this breach the children of Iſrael are ſaid to have entered the mountains, and not to have taken the moſt ſouthern road, which I think moſt probable: for thoſe valleys, to judge by what one now ſees, could not be paſſable for Pharaoh's chariots. This breach, the inhabitants told me, leads directly to a plain called Badeah, which in Arabic ſignifies ſomething *new* and *extraordinary*, and alſo *the beginning*, as the beginning of every thing is new, *i. e.* was not before known.

At Suez I found an opportunity of going to Tor by ſea, which I gladly embraced, that, by going nearer the place, at which the Iſraelites are ſuppoſed to have entered the golf, and having a view from the ſea, as well of that as of the oppoſite ſhore, I might be a little better able to form a judgement about it. Beſides, I was willing to have the views, bearings, and ſoundings, which I took, and they will appear ſome time or other; but this paper would ſcarce be their place, if I had them with me.

When we were oppoſite to Badeah, it ſeemed to me (for I was not on ſhore) a plain, capable of containing the Iſraelites, with a ſmall elevation in the middle of it. I ſaw ſomething too like ruins. The captain and pilots told me, that this was the place, where the Iſraelites entered the ſea, and the ruins were thoſe of a convent (I ſuppoſe built on the ſpot in commemoration of the fact); they added that there was good water there. There is here a ſtrong current, which ſets to the oppoſite ſhore, about ſouth eaſt; it forms

by its ſtrength a whirlpool, where ſailors ſaid ſhips were loſt, if forced into it, for want of wind, by the current. This pool is about ſix miles northward of Cape Karondel; and juſt below this pool there is a ſand, a flat iſland at low water, which runs eaſt and weſt about three miles. This ſand, I ſuppoſe, is thrown up by the force of the current; and the ſame current, by the reſiſtance it meets with from this bank, being forced back into the cavity made by this excavation, forms the whirlpool. This pool is called Birque Pharaone, the well, or pool of Pharaoh; and here they affirm his hoſt was deſtroyed. I ſhall ſay more of this as I travel back by land. We came to an anchor in fifteen fathom water, within a mile and a half of the ſhore, to the ſouthward of this ſand, and in the Birque Karondel, to the northward of the cape; here the eaſtern ſhore is already mountainous, which, near this place, was a ſandy beach: the Egyptian ſhore, from Suez to Badeah, is likewiſe rocky and ſteep; ſo no entering upon the golf from that ſhore, but at Badeah or Suez.

It is high water always, when the moon is at her meridian height, and it ebbs ſix hours. At Suez, it flows ſix foot; the ſpring tides are nine, and in the variable months, from the beginning of November, to the end of April, ſometimes twelve. From the beginning of May to the beginning of October, a northerly wind generally riſes, and goes down with the ſun; it is often very ſtrong. This wind never fails in theſe months, unleſs there be ſome violent ſtorm; the reſt of the year the winds are variable, and when they blow hard at S. and S. S. E. theſe winds ſet up the ſea through the narrow ſtreight of Babel Mandel, and up this golf through its mouth, between

between Gebel El Zait, on the weſt ſide of this ſea, and the ſouthermoſt point of the bay of Tor, on the eaſt ſide of this weſtern branch of this ſea, where it is not above twelve or fourteen miles over. I ſuppoſe ſuch a wind, hindering the water from going out, cauſes this extraordinary encreaſe in the ſpring tides. We ſee the ſame thing happen with the ſame winds at Venice, both golfs running nearly in the ſame direction.

The Egyptian, weſtern, or Thebaic ſhore, from Badeah ſouthward to oppoſite Tor, on the eaſtern ſhore, is all mountainous, and ſteep; and at Elim, the northermoſt point of the bay of Tor, ends the ridge of mountains, which begin on the eaſtern ſhore of this weſtern branch at Karondel. I ſay nothing of Elim, or Tor, or the marine productions of this golf, as this paper is intended to give an account of Sharme, Meenah El Dzahab, Kadeſh Barnea, the ſtone which Moſes ſtruck twice, and the inſcriptions. I, however, muſt ſay, that, from this place, mount Sinai, properly called, cannot be ſeen; but only the ridge or groupe of mountains, in which it is, and which altogether form that part of this tongue of land called in general Mount Sinai. The garden of the Monks of Mount Sinai at Elim renders in dates, &c. 20.000 piaſtres per ann. or £ 2.500.

We from thence croſſed the plain, in about eight hours, and entered the mountains of Sinai. They are of granite of different colours. At the entrance of the narrow breach, through which we paſſed, I ſaw, on a large looſe granite ſtone, an inſcription in unknown characters, given, I think, by Dr. Pocock, biſhop of Oſſory; however, as the Iſraelites had no writing, that we know of, when they paſſed here, I did not think it of conſequence enough to ſtop for;

 the

the Arabs told me, it was relative to a battle fought here between Arabs, and indeed I do not ſee what point of hiſtory it can illuſtrate; beſides, there are not above five or ſix words. We arrived at the convent of Mount Sinai, after the uſual difficulties mentioned by other travellers, were received as uſual, and ſaw the uſual places, of which, however, I ſhall give the plans as well as elevations, which I took. I muſt ſay, that the Monks were far from owning to me, that they had ever meddled with the print of the foot of Mahomet's camel. I examined it narrowly, and no chiſſel has abſolutely ever touched it, for the coat of the granite is entire and unbroke in every part; and every body knows, that if the coat of leſs hard ſtones than granite is once deſtroyed, it never returns. It is a moſt curious Luſus Naturæ, and the Mahometans turn it to their uſe.

Meribah is indeed ſurprizingly ſtriking. I examined the lips of its mouths, and found that no chiſſel had ever worked there; the channel is plainly worn by only the courſe of water, and the bare inſpection of it is ſufficient to convince any one it is not the work of man. Amongſt the innumerable cracks in rocks, which I have ſeen in this, as well as other parts of the world, I never met with any like this, except that at Jeruſalem, and the two, which are in the rock Moſes ſtruck twice, of which hereafter.

I had enquired of the captain and the two pilots of our ſhip, about Sharme and Dzahab, on the weſtern ſhore of the eaſtern branch of the Red-ſea; they told me that they were often forced up the Elanitic golf, the eaſtern branch of the Red-ſea, and generally went to Sharme,

Sharme, and ſometimes as high as Dzahab; that they generally ran from Cape Mahomet, the ſouthermoſt part of the peninſula between thoſe two golfs, to Sharme, in ſix hours, becauſe they always made as much more way, as they commonly do, they very ſeldom going there but in a ſtorm: they generally run four knots, ſo this makes forty eight miles, which brings it to the northward of Tor. Tor is in lat. 27. 55. Cape Mahomet thirty miles ſouthward, lat. 27. 25. Sharme forty eight miles nearly N. lat. 28. 13. conſequently about E. N. E. of Sinai. The port is pretty large, ſurrounded with high mountains, the entrance very narrow, and the water deep quite to the rocks, which are ſo very ſteep, that a ſtone dropt from the ſummit falls into the baſon. No wind can be felt here; they don't caſt anchor, but faſten their cables to the rocks. There is good water; ſome habitations are found on the ſides of the mountains, and a pretty large village at top: this ſeems to anſwer the idea of Neſt-Ken. Dzahab lies as high again up the golf, ſo forty eight miles more, or in lat. 29. This port is conſiderably larger than the former, and very good, but not ſo cloſely ſurrounded with mountains; it is however, very ſafe. There is a well of great antiquity with very good water; very conſiderable ruins are found, and they ſay, there was a great city formerly; but no habitations now, except an Arabian camp of 2000 men. There is a road from it to Jeruſalem, formerly much frequented. Thus far the captain and pilots. I enquired from the Monks, as well as Arabs, about theſe places, as well as about the ruins, ſuppoſed by my learned friend, the biſhop of Offory, to be Kadeſh Barnea: the former could only tell me, they had not received any fiſh from thence

thence in many years, that it was two eaſy days journey off, but the road was mountainous; ſo one may ſuppoſe the diſtance leſs than forty miles. The Arabs agreed as to the road; but they ſaid, it was once a large place, where their prince lived, whoſe daughter Moſes married, that Moſes was afterwards their prince, and the greateſt of all prophets. Theſe Arabs place Moſes the firſt, Salomon the ſecond, Mahomet the third, Chriſt the fourth, and then the prophets of the Bible. As to Dzahab, the Monks only knew the diſtance to be four days journey, and that there was a road from it to Jeruſalem: the Arabs told me the ſame, ſo the diſtance is about eighty miles. I enquired of them all about the ruins; they told me there were very conſiderable ones about half way to Dzahab, about forty miles from Sinai; but I ſhould think Kadeſh muſt have been much nearer to Jeruſalem. I would willingly have gone to theſe places; but as the four clans of Arabs, which inhabit this promontory, were then at war one with the other, I could get no conductor. In another journey I hope to be more lucky, for this is all hearſay; however, combining the whole together, and comparing it with what we collect from Scripture, I think we may well conclude, Sharme to be Midian, and Meenah El Dzahab to be Eziongeber: what the interjacent ruins are I cannot conjecture; but I believe I have found Kadeſh Barnea to be elſewhere. I think it cannot be here, for the Iſraelites were on the borders of the Holy Land, or Land of Promiſe, when they were ordered back; and when they were ſtopped by the Moabites, they are ſaid to have been brought up from Kadeſh Barnea; and I meet with no place in ſacred

facred writing, or any antient geographer, neither Strabo nor any other, that draw the line of divifion between this promontory and the Land of Promife fo low down; nor could they do it, as thefe ruins are within almoft feventy miles of the extremity of it. There are two roads from Mount Sinai to Jerufalem, the one through Pharan, the other by the way of Dzahab: that through Pharan is eleven days journey; two to Pharan, three to a ftation of the Mecca Pilgrims called Scheich Ali, one and an half to fome confiderable ruins; all this to the northward: from thence four and fomething more to Jerufalem, by way of Hebron, leaving the Afphaltic Lake on the right hand to the foutheaftward. The other way is longer, on account of the road being more mountainous; that too paffes the fame ruins, and alfo Scheich Ali. I enquired about this, when I was at Jerufalem, and received the very fame account, with this addition, that fuch Mahometans, as went from Jerufalem to Mecca, went that way, to join the Cairo caravan at Scheich Ali. This feems to be a fituation oppofite to Kadefh Barnea; at the line drawn by all the geographers; it is without Mount Sinai (taken for this whole tract); and juft before the Moabites, as the children of Ifrael paffed by Mount Hor, now Acaba, leaving the Afphaltic Lake on their left hand, to the northweft. The tradition too of the Arabs is, that they paffed this way; therefore, I think, Kadefh Barnea muft be near this fpot. There are here confiderable ruins; and I know of no city that ever was here, for Petra lay more to the eaft, between the Afphaltic lake and the Elanitic golf. To leave no enquiry wanting, I afked the Rabbins of Jerufalem, where they placed Kadefh Barnea; and they faid, thefe ruins.

We

We ſet out from Mount Sinai by the way of Scheich Salem; and, after we had paſſed Mahomet's ſtone, came to the beautiful valley, mentioned in the Journal. I lay there (and hope I have diſcovered the manna, but that will be the ſubject of another paper) and did not ſet out before day-light, that I might not paſs the rock which Moſes ſtruck twice. I ſearched, and enquired of my Arabs, but could neither hear nor ſee any thing of it. I ſaw ſeveral ſhort inſcriptions ſtained on ſome parts of the mountains, the characters being the ſame with thoſe on mount Sinai, Meribah, &c. given by the biſhop of Oſſory. About four miles before we arrived at Pharan, we paſſed through a remarkable breach in a rock; each ſide of it is perpendicular as a wall, about eighty feet high, and the breach is about forty broad. It is at this breach, I imagine, the Horites were ſmote, four miles beyond the preſent ruins of Pharan; for having paſſed this breach they could make a ſtand, nor could they well be purſued. Here, on the tops of the mountains to our right hand, were ruins of buildings, and one ſeemed a caſtle. From Meribah to near this place, we had always rather deſcended; in moſt places there is the bed of a ſtream and after rain the water runs; but a little before we came to this breach, it winded off towards the weſt, for the waters fall into that part of the deſert we croſſed from Tor. Between this breach and Pharan, there are ſeveral ſprings, and one at Pharan where we encamped; there is the bed of the river mentioned by the Journal, the traditional account of which agrees with what is ſaid by St. Paul. Waters ſeem to have run from Meribah to within about ſix miles of this place; the bed of a ſtream is here again very plain and a ſpring at the upper

upper end of it, which does not yield water enough to make a ſtream, the bed then is dry; four valleys terminate here, and form a large area. I enquired about the road to Jeruſalem; the people agreed in the diſtance and ruins. We travelled in the bed of the river through the valley to the north; and in about half an hour, the ſight and appearance of a large ſtone, not unlike Meribah, which lay at ſome diſtance from the mountain on our right hand, ſtruck me; and I alſo obſerved, it had many ſmall ſtones upon it. The Arabs, when they have any ſtone or ſpot in veneration, as Mahomet's ſtone, and the like, after their devotion, lay ſome ſmooth ſtone upon it. I aſked what it was, they told me Hagar Mouſa, the ſtone of Moſes. I told them that could not be, for that lay in Rephidim; they ſaid that was true, but this was Hagar il Chotatain, the ſtone of the two ſtrokes; that he ſtruck it twice, and more water came from it than from Meribah; witneſs the river. The bed of the river winds to the eaſtward, about E. S. E. I aſked how far it went; they ſaid this bed ran by Sheich Ali to thoſe ruins, and quite away to the ſea; ſo the river muſt have begun here, and not at Pharan, and the bed from Pharan here is only formed (I ſuppoſe) by winter torrents. If this is the bed of the river mentioned by St. Paul, as I dare ſay it is, we have the ſecond rock: if it runs to the ruins, as is ſaid, and there is no reaſon to doubt it, they will be pretty plainly thoſe of Kadeſh Barnea; and if this bed continues in the ſame courſe to the ſea, as it probably does, this probably is the river at Rinocolura, ſuppoſed, by Eratoſthenes, to be formed by the Arabian lakes; becauſe he did not know its miraculous

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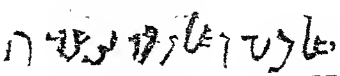
head. This river is doubted of by Strabo, becauſe dried up to the ſource, from the time the Iſraelites entered the Land of Promiſe, and the tradition was then loſt. You may ſee Strabo's Aſſyria, edit. Caſaubon, p. 5. 10. towards the bottom. Pardon this bold conjecture; but it coincides and conciliates ſacred hiſtory with antient geography. This too ſeems a proof, that this is really the ſecond ſtruck rock. As to the ſprings between the breach and Pharan, they certainly did not exiſt in the time of Moſes; or, if they did, they would have been as nothing to ſo many people.

We went down a large valley to the weſt, towards the ſea, and paſſed the head of a valley, a part of the deſart of Sin, which ſeparates the mountains of Pharan from thoſe which run along the coaſt, and the ſame plain, which we had paſſed from Tor. We had ſcarce entered theſe mountains, and travelled an hour, when after paſſing a mountain, where there were viſible marks of an extinguiſhed ſubterraneous fire, we ſaw, on our left hand, a ſmall rock, with ſome unknown characters cut on it, not ſtained upon it, as thoſe hitherto met with; and, in ten minutes, we entered a valley ſix miles broad, running nearly North and South, with all the rocks, which encloſe it on the Weſt ſide, covered with characters. Theſe are what are called Gebel El Macaatab, the written mountins. On examining theſe characters, I was greatly diſappointed, in finding them every where interſperſed with figures of men and beaſts, which convinced me they were not written by the Iſraelites; for if they had been after the publication of the law, Moſes would not have permitted them to engrave images, ſo immediately

 after

after he had received the ſecond commandment: if they went this way, and not along the coaſt, they had then no characters, that we know of, unleſs ſome of them were ſkilled in hieroglyphicks, and theſe have no connection with them. It will be difficult to gueſs what theſe inſcriptions are; and, I fear, if ever it is diſcovered, they will be found ſcarce worth the pains. If conjecture be permitted, I will give my very weak thoughts. They cannot have been written by Iſraelites, or Mahometans, for the above reaſon; and if by Mahometans, they would have ſome reſemblance to ſome ſorts of Cuphic characters, which were the characters uſed in the Arabic language, before the introduction of the preſent Arabic letters. The firſt MSS. of the Alcoran were in Cuphic: there is a very fine one at Cairo, which I could not purchaſe, for it is in the principal Moſque; and the Iman would not ſteal it for me, under four hundred ſequins, £200. Theſe have not the leaſt reſemblance to them: Saracen characters are very unlike; beſides, I ſhould place them higher than the Hegira. I think it then not unprobable, that they were written in the firſt ages of Chriſtianity, and perhaps the very firſt; when, I ſuppoſe, pilgrimages from Jeruſalem to Mount Sinai were faſhionable, conſequently frequent and numerous, by the new Chriſtian Jews, who believed in Chriſt; therefore, I ſhould believe them Hebrew characters, uſed vulgarly by the Jews about the time of Chriſt. I ſhewed them when at Jeruſalem to the Rabins; they were of the ſame opinion, and thought , which is frequent, was שלם; and to that , which is juſt before with a

 ſmall

ſmall croſs שלב שך ישוע, by changing the *ſhin* into *ſin*, and adding *je*, it would be an Arabic word ساليبن a croſs, and might be explained, the croſs borne or carried by Jeſus. The Hebrew would be Jeſus brought ſafety, or ſalvation. But, Sir, more able than me will judge better. Theſe are all conjectures; and it ſeems much eaſier to ſay what theſe inſcriptions are not, than what they are. They can ſcarce be of St. Helen's time; for they would have ſome analogy with Greek characters, and they have none. Perhaps ſome gentlemen will think them antient Egyptian, written, by the colony, which they ſuppoſe went to inhabit China. That is a matter I won't meddle with; but, amongſt many others, it will be liable to one great objection, which is, that ſuch colony, if there ever was one, probably went the ſtreight road, from the head of one golf to the head of the other, from Hierapolis to Eloth, the way the Mecca pilgrims now go. This place would have been far out of their way, being at leaſt ſixty miles to the ſouthward of the pilgrims road, unleſs they were ſuppoſed to have had tranſports at Dzahab, or Sharme. I, for the firſt reaſon given, did not think them written by the Iſraelites, and could not conceive that they were of any great conſequence. I only took theſe few as a ſpecimen. Here are on other parts of this rock, ſome Greek, and Arabic, as well as ſome Saracen inſcriptions, and an Hebrew one, which is ושבר אחד. The Saracens and Arabic only ſay, " ſuch an one was here at ſuch a time"; the ſame ſay the Greek ones, except one, which ſays, as I remember, for I have it not with me, " The evil genius of the army wrote this," which can only prove, that ſome body of Greeks was worſted here, after the charactere were

were written, and that they attributed their defeat to ſome magick power in theſe characters: as we are now fruitful in conjecture, perhaps ſome gentlemen will bring Xenophon here. The characters ſeem to be of the very ſame kind with thoſe ſtained on different parts of Mount Sinai, Meribah, &c. which my learned and accurate friend the Biſhop of Offory has given.

The third day from this place, travelling weſtward, we encamped at Sarondou, as the Journal calls it; but it is Korondel, where are the bitter waters, Marah. I tried if the branches of any of the trees had any effect on the waters; but found none: ſo the effect mentioned in Scripture muſt have been miraculous. Theſe waters at the ſpring are ſomewhat bitter and brackiſh, but as every foot they run over the ſand is covered with bituminous ſalts, grown up by the exceſſive heat of the ſun, they acquire much ſaltneſs, and bitterneſs, and very ſoon become not potable. This place, off which the ſhips caſts anchor, is below the ſand, which I mentioned before, near the Birque Korondel. After nine hours and a half march we arrived, and encamped at the Deſert of Shur, or Sour. The conſtant tradition is, that the Iſraelites aſcended from the ſea here; this is oppoſite to the plain Badeah, to which the abovementioned paſs in the mountains lead. From this place the openings in the mountains appear a great crack, and may be called a Mouth, taking Hiroth for an appellative. However, I ſhould rather adopt the ſignification of Liberty. It would hardly have been neceſſary for the Iſraelites to paſs the ſea, if they were within two or three miles of the northern extremity of the golf; the ſpace of at moſt two miles, the breadth of

of the golf at Suez, and at moſt three foot deep at low water, for it is then conſtantly waded over, could not have contained ſo many people, or drowned Pharaoh's army. There would have been little neceſſity for his cavalry and chariots to precipitate themſelves after a number of people on foot, incumbered with their wives, children, and baggage; when they could ſoon have overtaken them with going ſo little about. Theſe reaſons, added to the ſignificant names of the places, Tauriche Beni Iſrael, road of the children of Iſrael; Attacah, Deliverance, Pihahiroth, whether an appellative or ſignificative; Badeah, new thing, or miracle; Bachorel Polſum, ſea of deſtruction; convince me, that the Iſraelites entered the ſea at Badeah, and no where elſe. Beſides, all the reſt of the coaſt from Suez, and below Badeah, is ſteep rocks, ſo there muſt have been another miracle for them to deſcend: the current too ſets from this place where we encamped, toward the oppoſite ſhore, into the pool Birque Pharaone, Pool of Pharaoh, where, the tradition is, his hoſt was drowned: a current, formed, I ſuppoſe, by the falling and ruſhing of one watery wall on the other, and driving it down: a current, perhaps, by God permitted to remain ever ſince, *in memoriam rei*; the diſtance to the bitter waters is about thirty miles. I omitted to mention in its place, that, between this and Korondel, we were not ſo lucky as the author of the Journal, who met with a charming rivulet of ſweet water; we met with none, good or bad. The Ain Mouſa, which the Iſraelites would have met with, if they had paſſed at Suez, and the coaſt from hence ſouthward, about a mile to Tor, being all rock and ſteep too, induce me to believe, that they entered

ed the ſea at Badeah, and aſcended from it here, and not at any other place. But I am too ſenſible of my own inability to decide, and leave that to better judges than I am. I only throw out what occurs to me, from the inſpection of the country, an inſpection as accurate as I am capable of. If any thing I have ſaid can in the leaſt ſupport that revelation, to which I dare declare myſelf a friend, even in this enlightened age, I ſhall be very happy; or if this trip of mine can be of any uſe whatever, as I had great pleaſure in it, I may truly ſay with Horace—*Omne tulit punctum*, &c.

The denomination of ים סוף, I believe, only regards the Hierapolitic branch, as the marine productions, Madrepores, &c. which form admirable foreſts in the bottom of it, are not in the Elanitic branch, or the golf; I mean the broad part below Cape Mahomet. No more than that weſtern branch was known to the Iſraelites at the time of their paſſage, if it was to the Egyptians: but the name deſcended to the whole, as their knowledge of it. The Red Sea ſeems to regard the broad part alone; for tho' there are not the abovementioned ſea productions, yet there is ſo great a quantity of tube coral (not found in the weſtern branch of the Hierapolitic golf) and ſuch rocks, as one may ſay of them, that the Gedda ſhips faſten themſelves to them inſtead of caſting anchor. It is of a deep red, ſo that poſſibly, the firſt navigators entering at the ſtreight of Babel Mandel, from the red they ſaw, called it the Red Sea, and that name deſcended to the whole with their navigation. This ſea is tempeſtuous and full of ſhoals; there is no harbour on the Arabian coaſt after Tor, except one, I mean between Suez and Gidda or Mecca, which is a day and a half from

Gidda:

Gidda. Gidda is its port; and there is only one on the other coaſt, Coſſire; but it is a very bad one; however, ſhips ſometimes go thither, and caravans croſs the country to Morſhout. The ſhips are, as the biſhop of Oſſory has deſcribed them; the helm is on the outſide, as I ſuppoſe, with his lordſhip, that of St. Paul was. They make uſe of but four ſails, and no compaſs, nor do they ever caſt the lead. They ſail only by day light, from anchoring place to anchoring place and are not, above two days out of ſight of land, from Cape Mahomet to the Arabian main: if a gale happen, they are often loſt; about one in ten every year. I ſhall be glad to be honoured with the Society's command, and in communicating this, you will oblige,

Sir,

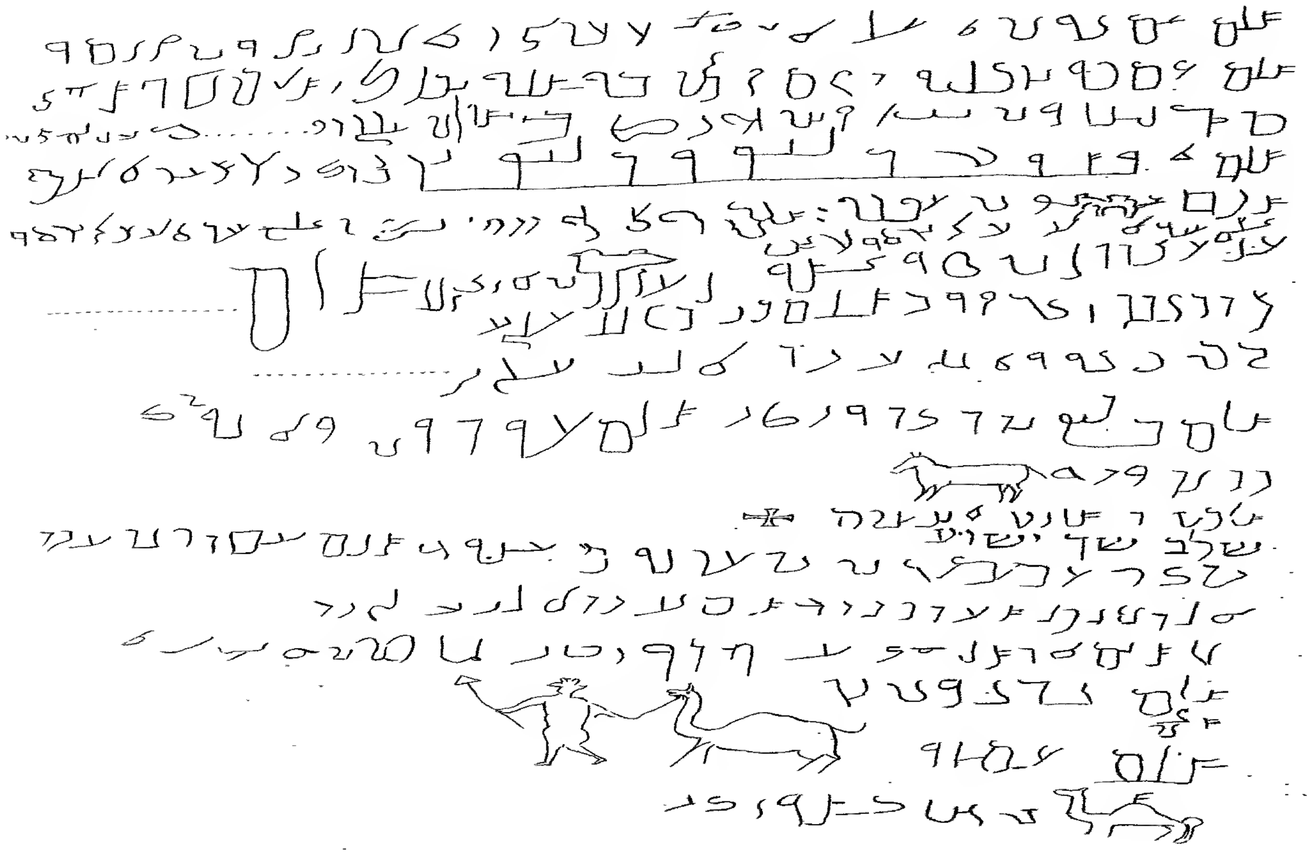
Your moſt humble ſervant,

Piſa, Dec. 2, 1765.

Ed. Wortley Mountagu.

P. S. I am a very bad draughtſman; but I aſſure you, the ſketches contained in Plate III. are rather better than the originals. They are about ſix inches long, the marble is whitiſh, in ſome places reddiſh, of a fleſh color; they are engraved with a pointed inſtrument, for one ſees, in the bottom of them, round marks of the point of the inſtrument. I have met with much baſalto, but not one piece of that ſoft ſtone of which is the buſt at Turin, nor any of the characters upon it, except ſome are found amongſt theſe, I have neither ſeen any head, buſt, or ſtatue, in the character of that.

The



The ſecond rock ſtruck by Moſes is, I think, 43 feet long, 16 broad, 13 high; it has two cracks, oblique ones; in them are ſome mouths, like thoſe of Meribah: it is of a hard ſtone, not granite or marble.

I have the exact dimenſions and elevation of the ſecond ſtone, as well as of Meribah.

IX. *A Diſcovery, with Obſervations, of two new Comets in the Marine Obvatory at* Paris; *by* M. Meſſier, *F. R. S. and Member of the Academy of Sciences at* Paris; *tranſlated from the French, by* M. Maty, *M. D. Sec. R. S.*

Read March 20, April 24, and May 15, 1766.

ON the 8th of March 1766, the ſky having been clear the whole day, I had a mind to make uſe of this fine weather to look for the ſatellite of Venus, which for ſome years has been talked of. I employed, for this purpoſe, an excellent Gregorian teleſcope, of 30 inches focus; the great ſpeculum of which, being ſix inches diameter, magnified objects about 109 times. I could diſcover nothing with this inſtrument, the planet appearing only ſurrounded with ſmall teleſcopic ſtars. I likewiſe made uſe of a very good achromatic teleſcope of 5 feet, conſtructed at Paris, and belonging to the Pr. of S. with which I diſcovered at about 7 o'clock, and at ſome diſtance of Venus, a nebuloſity of a ſmall extent,

 with

with a luminous center. The time did not permit me that day to assure myself, whether it was a comet or a nebulous star. I was only able, before its setting, to take its position, by comparing it with a star of the 4th magnitude, and to defer to the next day the compleating of this discovery. This very night, after the observation, I looked over my copy of the celestial maps of Mr. Flamstead, upon which I have delineated all the nebulous stars, which I have discovered for some years, and found one in that part of the heaven, which I began to see August 25, 1764. My description is thus entered in my journal: "I applied myself to "the discovery of nebulous stars, the night between "the 25th and 26th of August 1764, and I discover-"ed one near the star α of the great triangle, which "I compared with that star, in order to obtain its "position. This nebulous star is a whitish spot "15 minutes in diameter; the light nearly uniform, "tho' somewhat brighter on the right side; it is seen "with difficulty by a common refracting telescope of "one foot." Imagining, that what I had then been looking at was this nebulous star, I lost all hopes for the next day. Being, however, impatient to observe the sky, I found in the evening, that this nebulous star had altered its position, being got nearer to the star, with which I had compared it the day before, and which I then found to be the star η of the knot of Pisces of the 4th magnitude. I determined the position of the comet with respect to this star, with all the care imaginable, by means of a Newtonian telescope of $4\frac{1}{2}$ feet in length, furnished with a silken-threaded micrometer. This is the table of my observations.

A Table of the Places of the firſt Comet of 1766, obſerved during Part of the Month of *March*, deduced from the Determination of its Poſition, with reſpect to certain Stars.

1766.	tr. Tim. h	′	″	Rig. Aſc. obſerv. °	′	″	Dec. Nor. obſerv. °	′	″	Stars with which the Comet was compared.
Mar. 8	7	34	22	17	39	46	14	58	11	Com. comp. with η Piſc.
9	6	55	4	18	42	46	14	34	40	——— with the ſame
	7	35	28	18	44	46	14	35	3	——— with the ſame
	7	47	8	18	45	37	14	31	4	——— with the ſame
10	6	49	52	19	45	1	14	10	38	——— with the ſame
	7	0	23	19	45	31	14	10	37	——— with the ſame
	7	9	30	19	45	46	14	10	2	——— with the ſame
	7	40	58	19	47	16	14	9	33	——— with the ſame
11	6	58	47	20	44	46	13	46	35	——— with the ſame
	7	21	37	20	45	31	13	46	10	——— with the ſame
	7	27	0	20	45	36	13	46	24	Com. with Flam. 31 of Piſc.
	7	35	21	20	46	6	13	46	13	——— with the ſame
12	6	56	28	21	39	43	13	23	45	——— with the ſame
	7	5	47	21	40	13	13	23	48	——— with the ſame
	7	20	38	21	40	43	13	23	52	——— with the ſame
	7	41	48	21	42	16	13	22	51	——— with η Piſc.
	7	51	26	21	44	51	13	23	58	——— with 101 Piſc.
13	6	49	46	22	31	21	13	1	46	——— with the ſame
	7	34	17	22	32	1	13	0	19	——— with η Piſc.
	7	54	46	22	36	1	13	0	18	——— with the ſame
14	6	53	0	23	22	16	12	39	40	—with Flam. 104 Piſc.
	7	25	16	23	22	46	12	38	46	——— with η Piſc.
	7	47	44	23	25	1	12	38	33	——— with the ſame
15	7	6	3	24	5	51	12	18	2	—with ⁎ above 101 Piſc.
	7	34	16	24	8	51	12	17	27	——— with the ſame
	7	58	57	24	9	21	12	17	7	——— with the ſame

TABLE of the Poſitions of the Stars, with which the Comet was compared, reduced to the time of the Obſervations.

Rig. Aſc.			Dec. Nor.			
19	45	1	14	8	0	*n* of the knot Piſc. Comet comp. 8, 9, 10, 11, 12, 13, & 14.
20	49	6	13	28	30	101 Piſc. of Flamſtead Com. comp. the 11, 12, 13, & 15.
21	41	24	13	6	30	104 of Piſc. Comet compared the 14.

From theſe Obſervations, Mr. Pingré has computed the Elements of the Orbit of this Comet, as follows.

		s	°	′	″
Place of the aſcending node ☊	—	8	4	10	50
Inclination of the orbit	—		40	50	20
Place of the perihelium	—	4	23	15	25
Logarithm of the perihelion diſt.		9.703570			

The comet paſs'd its perihelion, the 17th of February, at 8^h 50′, mean time at the meridian of Paris.

The motion of the comet retrograde.

Obſervations of the ſecond Comet of 1766, *diſcovered at the Marine Obſervatory at* Paris, *one month after the former, viz. the 8th of* April. *By* Mr. Meſſier, &c.

The 8th of April, the ſky having cleared up after many days of cloudy weather; on the evening at 8 o'clock, being gone to the Marine Obſervatory, to obſerve ſome tranſits of ſtars on the meridian, and looking on the heavens towards the weſt, I diſcovered, by my naked eye, near the horizon, and at a little diſtance from the Pleiades, a comet, which already appeared conſiderable; the tail was about 4 degrees in length, the light lively, the nucleus very bright and equally luminous with ſtars of the 3d magnitude. The comet was at a ſmall diſtance from the brighteſt ſtar of the conſtellation of muſca, which Flamſtead, in the ſecond

ſecond edition of his catalogue, makes of the 3d magnitude. I ſeveral times compared the nucleus of this comet with this ſtar, to deduce its poſition. The next day, being the 9th, the ſky appearing perfectly ſerene without a moon, I began to ſee the comet a few minutes before 8 o'clock. During one hour, it was diſtinctly ſeen with the naked eye, the tail was 6° or 7° in length, the nucleus very bright. I meaſured its diameter, by comparing it with the thickneſs of one of the threads of the micrometer of my inſtrument, and found it about 36″ of a degree. On the 10th, the ſky being equally bright, I ſaw the comet ſome minutes before 8 o'clock, and between 8 and 9 it was ſeen with the naked eye, with a longer tail than yeſterday, but not ſo diſtinct; the nucleus had alſo loſt part of its brightneſs. On the 11th, the ſky being clear as the preceding days, the comet could only with difficulty be ſeen with the naked eye; its appearances were much leſs diſtinct than the day before, both on account of the vapors of the horizon where it was, and of the twilight which was conſiderable, and ſtill more ſo by the light of the moon, which, as well as the two preceding days, was in the part of the heavens, where the comet appeared. On the 12th, the comet could no longer be ſeen with the naked eye. Through the teleſcope it appeared very faint, the tail not exceeding now one degree and a half. It certainly became thus inviſible, as well as the nucleus, through the too great power of the twilight. The 13th, the ſky was ſerene, with ſome clouds towards the horizon; I looked for the comet, but could not find it, ſo that the evening of the 12th was the period of its viſibility.

TABLE of the Obſervations of the Comet.

1766.	tr. Tim. h	′	″	Right Aſc obſerv. °	′	″	Dec. Nor. obſerv. °	′	″	Stars with which the Comet was compared.
Apr. 8	8	33	54	39	29	56	25	12	16	Com. com. with the Star 41 of Aries, according to Flamſtead.
	8	57	40	39	25	41	25	11	23	Com. com. with the ſame
	9	6	43	39	24	41	25	10	22	——— with the ſame
	8	7	7	37	59	26	24	26	5	——— with 33 of Aries
	8	7	7	37	59	41				——— with new Star of 7 Mag.
	8	21	40	37	59	18	24	25	5	——— with 33 of Aries
	8	21	40	37	59	41				——— with new Star of 7 Mag.
	8	33	4	37	58	41	24	24	20	——— with 33 of Aries
	8	33	4	37	58	41				——— with new Star of 7 Mag.
	8	44	46	37	58	56	24	25	3	——— with the ſame
	8	44	46	37	58	41	24	24	32	——— with new Star of 6 Mag.
	8	4	15	36	31	15	23	36	46	——— with ε of Aries
	8	16	35	36	30	34	23	36	2	——— with 30 of Aries
	8	16	35	36	30	41	23	36	45	——— with new Star of 7 Mag.
	8	16	3.	36	30	56	23	36	49	——— with the Star of 6 Mag.
	8	36		36	29	11	23	36	1	——— with new Star of 7 Mag.
	8	36	26	36	28	56	23	35	31	——— with the Star of 6 Mag.
11	8	6	57	34	57	53	22	33	38	——— with α of Aries
12	7	39	5	33	29	23	21	39	16	——— with the ſame
	8	2	11	33	28	46	21	39		——— with the ſame

TABLE of the Poſitions of the Stars, with which the Comet was compared, reduced to the Time of the Obſervations.

Right Aſc. °	′	″	Dec. Nor. °	′	Mag	
39	3	41	26	17		41 of ♈ accor. to Flam. ded. fr. A. de la Caille's Cat.
38	48	56	24	16	25	It is only men. in Flam. Select M. I deter. its Poſit.
37	35	11	24	40	51	New. I determined its Poſition
36	46	26	26	2	0	33 of ♈ accor. to Flam. I determined its Poſition
35	51	34	23	46	6	30 of ♈ accor. to Flam. I determined its Poſition
28	30	38	22	20	55	α of ♈ deduced from Abb. de la Caille's Catalogue

From

From my obſervations, M. Pingré has calculated the elements of this Comet as follows;

				″
Place of the aſcending node	1	17	22	19
Inclination of the orbit		8	18	45
Place of the perihelion	6	26	5	13
Perihelion diſtance		0,636825		
Logarithm of the perihelion diſtance		9,804020		

It paſſed the perihelion April 17, $0^h\ 26'\ 13''$ mean time at the meridian of Paris.

The motion of the Comet direct.

By theſe elements M. Pingré judged that this Comet might be ſeen again in the morning, after getting clear of the ſun's rays; and the following is an Ephemeris he computed for finding it in the months of May and June.

An Ephemeris of the second Comet of 1766, for the months of May and June, at 16^h mean time at the meridian of Greenwich.

May		Com. rises.		Comet's Long.		Com. Lat. S.		Com. R. Asc.		Com. Dec. S.		Comet's Dist. fr. Earth.	Comet's Dist. fr. Sun.
d	h	h	′	°	′	°	′	°	′	°	′		
1	10	15	52	♈ 5	2	3	43	6	3	1	23	0,437	0.709
3	16	15	45	3	0	4	37	4	37	3	3	0.455	0,729
5	16	15	39	1	19	5	26	3	24	4	28	0,473	0,,60
7	16	15	32	♓ 29	54	6	10	2	23	5	42	0,492	0,773
9	16	15	26	28	41	6	51	1	32	6	48	0,510	0,797
11	16	15	20	27	39	7	28	0	50	7	46	0,528	0,821
13	16	15	14	26	47	8	1	0	16	8	38	0,546	0,847
15	16	15	8	26	3	8	32	359	47	9	24	0,564	0,874
17	16	15	2	25	24	9	1	359	23	10	6	0,581	0,901
19	16	14	55	24	49	9	28	359	3	10	45	0 597	0,929
21	16	14	49	24	18	9	54	358	45	11	29	0,613	0,957
23	16	14	42	23	49	10	18	358	29	11	53	0,628	0,986
25	16	14	35	23	22	10	40	358	13	12	25	0,642	1.016
27	16	14	29	22	56	11	2	357	58	12	56	0,656	1,046
29	16	14	22	22	31	11	23	357	44	13	26	0 669	1.076
31	16	14	15	22	7	11	42	357	30	13	56	0 682	1,105
June 5								356	48	15	3		
10								355	58	16	13		
15								354	57	17	24		
20								353	41	18	37		
25								352	10	19	53		
30								350	22	21	10		

In the months of May and June, I sought for the comet in the morning, when the sky was serene, in the places indicated in this ephemeris, without being able

able to discover it: the twilight, which was considerable at the time of the comet's rising, might hinder my seeing it, and I have not yet learned that it was seen any were else.

Mr. Pingré's remarks on the two comets of this year.

The elements of the first comet I give as absolutely certain, those of the second I cannot be so sure of. The interval was only four days between the first and the last observation. The two last days, and especially the last, the twilight and the moon light must have produced some uncertainty in the observation. The ephemeris is founded upon the certainty of the elements. Supposing there were no more than three or four minutes error in the two last observations, this would not much alter the theory from what I have given; but an alteration in the elements would produce two or three degrees difference in the place of the perihelion, which might be sufficient to render the reapparition of the comet uncertain in these high latitudes. Some German observations sent to Mr. Messier, made in the beginning of April, induce me to conclude, that the place of the perihelion ought really to be placed a little more eastward; but these observations were sent in so confused a manner, that it seems impossible to obtain the least light from them. Perhaps we may hereafter receive from some southern parts, observations sufficient to make out the true orbit of this comet.

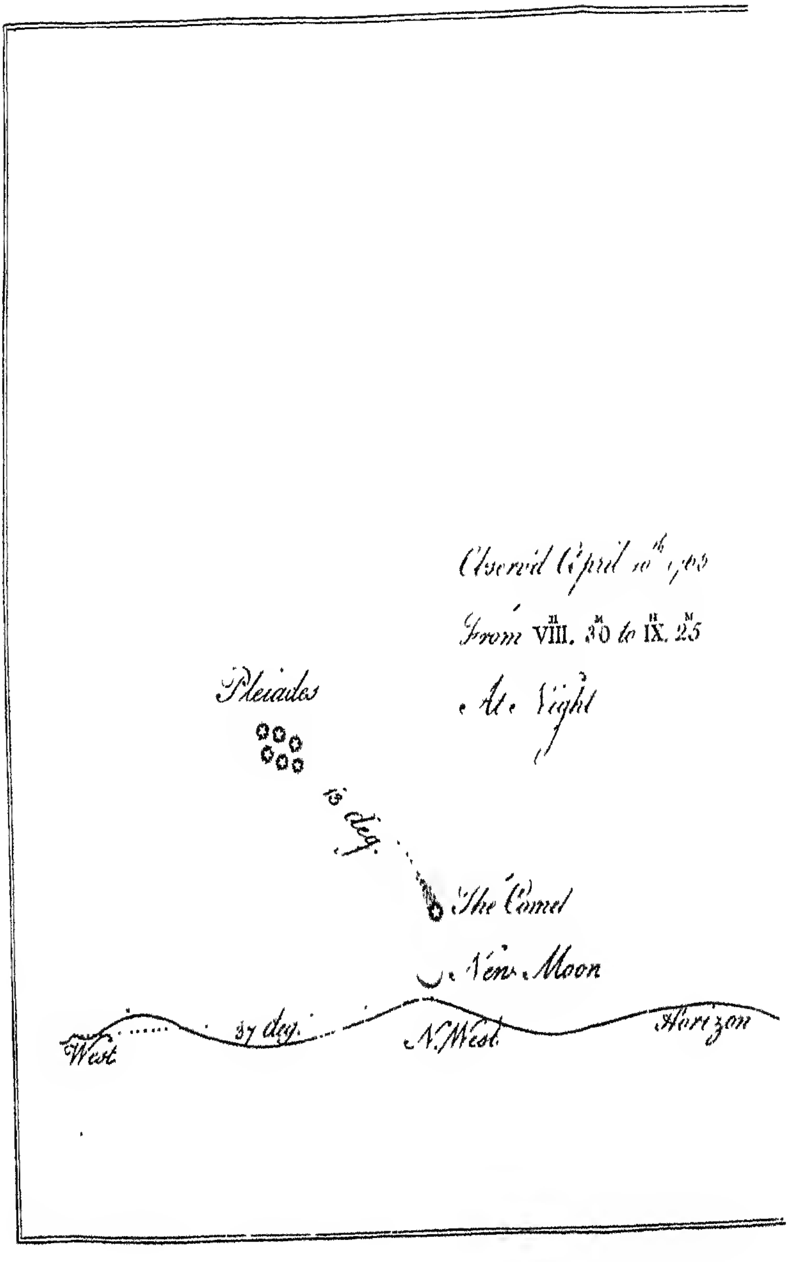
X. *A Letter from* Mr. Alexander Brice, *to the Earl of* Morton, *Prefident of the R. S. giving an Account of a Comet feen by him.*

Kirknewton, April 11, 1766.

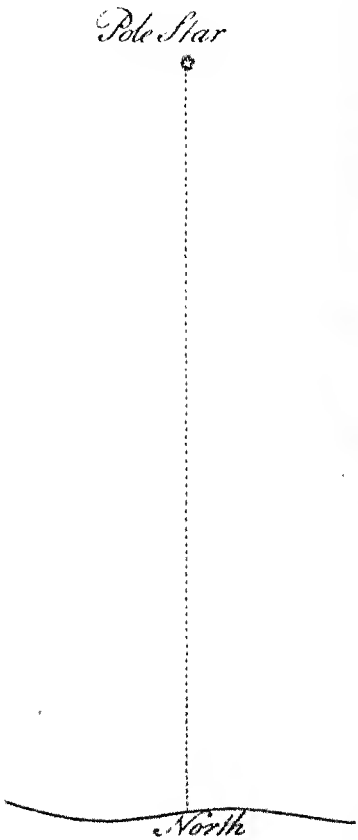
My Lord;

Read April 17, 1766.

I Beg leave by this, to acquaint your lordfhip, that, laft night and the night before, I obferved a comet in the northweft, and very near the horizon. It began to appear at half an hour after 8 o'clock, and fet 25 minutes after 9. The tail was very vifible to the naked eye; but the nucleus could not be feen by any, that were prefent with me, without a telefcope, thro' which it appeared very diftinctly, like a ftar of the 4th or 5th magnitude. It was furrounded with a gleam of light, like what is feen round the ftars in Orion's fword, comonly called Janua Cœli. The tail ftretched upwards, and inclined to the weft, and was about 4 degrees long: the body of the comet was alfo 4 degrees diftant from the new moon (then 34 hours after the change) and almoft perpendicular above it; and it appeared to more advantage after the moon was fet. The comet, when fetting, was 37 degrees to the north of due weft, and 13 degrees more northerly than the Pleiades, below them, but in the fame tract: it is defcending towards the fun, at the rate of 6 degrees as near as I could guefs, in the fpace of twenty four hours; and unlefs the light of the moon, or the twilight prevents, it will



Observ'd April 10th
From VIII. 30 to IX. 25
At Night
Pleiades
15 deg.
The Comet
New Moon
37 deg.
West
N. West
Horizon



J. Mynde sc.

will make a more remarkable appearance after it has paſſed the ſun, than it does at preſent. I know not whether this comet has been obſerved by any perſon about London; if I thought it had, I would not have given your lordſhip the trouble of this; but I wiſhed to put other people upon the look-out, who will probably make more of it than I can. [For the appearance of this comet, ſee Plate IV.] I have the honour to be,

My Lord,

Your Lordſhip's

moſt obedient and moſt humble ſervant,

Alex. Brice.

XI. *A Report concerning the Microſcope-Glaſſes, ſent as a Preſent to the Royal Society*, by Father* di Torre *of* Naples, *and referred to the Examination of* Mr. Baker, *F. R. S.*

Read April 17, 1766.

MR. Baker now returns the microſcope glaſſes, which the Royal Society did him the honour to refer to his examination: and he would have returned them much ſooner, had he not waited till he could examine them by bright day-light, being deſirous to do them all the juſtice in his power.

* Vid. Phil. Tranſ. Vol. LV. p. 258.

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They are globules of glaſs, formed over a lamp, by Father di Torre, and ingeniouſly placed in cells of braſs, adapted to Wilſon's microſcope. Four of theſe cells, thus furniſhed, were ſent as a preſent from the Father to this Society, under the care of our late worthy member Sir Francis Eyles Stiles: but when they came into Mr. Baker's hands, one of theſe minute glaſſes was wanting, having probably been ſhaken out of its cell in carriage: the loſs, however, ſignifies little, as there remains another of the ſame magnifying power.

Theſe globules are wonderfully ſmall: the largeſt being in diameter only two Paris points, and ſaid to magnify the diameter of an object 640 times: the ſecond is the ſize of one Paris point, magnifying the diameter of an object 1280 times; and the third is ſo extremely minute, as to be no more than one half of a Paris point, or the 144th part of an inch in diameter, and is ſaid to magnify the diameter of an object 2560 times, and conſequently it muſt magnify the ſquare of ſuch diameter 6,553,600 times.

Now as the focus of a glaſs globule is at the diſtance of $\frac{1}{4}$th of its diameter, it is with the utmoſt difficulty that globules ſo minute as theſe can be employed to any purpoſe. For inſtance, the focus of that globule, whoſe diameter is but one half of a Paris point (or the 144th part of an inch) is no farther from the object to be examined, than the 576th part of an inch. In attempting to find this focus, it is ſcarce poſſible to avoid touching the object with the glaſs, if it be not placed between laminæ of talc or iſinglaſs; and if it be ſo placed, even the thinneſt talc bears a conſiderable proportion to this 576th part of

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an inch, and will prove an unſurmountable obſtacle to the ſeeing any object, unleſs by ſome very happy accident.

The other globules, whoſe focus is not quite ſo near, are liable proportionally to the ſame inconvenience.

The very great magnifying power of glaſs globules is ſufficiently well known: many years ago they were much uſed, and highly boaſted of on that account. But they now, long ſince, have been laid aſide, and convex lenſes ſubſtituted in their room; and that with very good reaſon, from the difficulty in the application of ſuch globules, from the deficiency of light, from the diſtorſion of the image ſeen, from the painful ſtraining of the eyes, and from the boundleſs latitude given to imagination and conjecture, for want of ſufficient diſtinctneſs and preciſion.

Nothing can be more injudicious than the deſire of ſuch exceſſive magnifying power: whenever we can ſee an object clearly and well defined, we ought to be contented; all beyond this there is no dependence on.

In ſome letters, ſent with theſe glaſſes, the Society has been favoured with uncommon obſervations on the globules of the blood, deſcribed as having been viewed (it is not ſaid by theſe glaſſes) floating in the ſerum, and ſometimes changing their figure therein: and alſo with a long account of the impregnation of vegetables; wherein we are told, that the exquiſitely minute corpuſcles or ſeminal particles, emitted by the grains of the Farina fœcundans, have been ſeen to enter into, and be conveyed along tubes exceeding ſmall, which at the time dilated and contract-

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ed occaſionally to convey them to the ovarium *. Mr. Baker was extremely deſirous to repeat theſe experiments: but as it was abſolutely neceſſary to ſpread the blood as thin as poſſible, to render it very tranſparent, without which nothing can be ſeen by ſuch ſmall glaſſes, he could not poſſibly prevent its becoming quite dry, before he could apply it to the eye, and conſequently was unable to perceive any floating globules: and though he has been many years converſant with microſcopes, he has not been able to contrive any method of applying the parts of generation of plants in ſuch manner, to theſe glaſſes, as to view this wonderful impregnation.

* The curious will find the whole account, with copper-plates relating thereto, Phil. Tranſ. Vol. LV. p. 258—270; from whence one ſingle paſſage ſhall be here quoted, viz. p. 262. "The grains being arrived at a ſtate of maturity before they "iſſued from the antheræ, are prepared to burſt and diſcharge "their contents when they fall on the hairs: and the female "organ aſſiſts likewiſe in producing this effect; for ſoon after a "grain has lodged itſelf, the point of the hair begins to open, "and the mouth extends itſelf by degrees over the ſurface of the "grain, till almoſt the whole body of the grain is drawn within "the tube; in this ſituation the grain ſoon yields to the com- "preſſion of the tube, and diſcharges its corpuſcles, which, with "the aſſiſtance of the fluid parts of the pulp that enter with them, "or of the juices with which the tube itſelf is furniſhed, float on "till they enter the longitudinal ducts, which convey them to "the germen."—It muſt be obſerved here, in juſtice to Mr. Turberville Needham, F. R. S. that he was the perſon who firſt diſcovered, that, on applying water to the Farina fœcundans, many of its grains emitted ſtreams of exquiſitely minute globules, as if through a ſmall aperture: this he publiſhed in the year 1745 †, and from thence imagined the impregnation of plants to be carried on in a manner ſomewhat ſimilar to that in the account referred to; but the ſame juſtice muſt allow, that, before Father di Torre, nobody is ſuppoſed to have ſeen theſe ſeveral progreſſions towards impregnation.

† Vid. Microſcopical Diſcoveries by Mr. Needham, p. 73, &c.

It is, however, proper to take notice, that in these letters an apparatus is defcribed, to be added to Wilfon's microfcope, when thefe glaffes are made ufe of; which apparatus Mr. Baker was not at the expence of procuring, as it would anfwer no other purpofe: but the method he contrived, inftead thereof, he imagines to be equally effectual.

In truth, Mr. Baker has employed much time and his beft endeavours in the examination of thefe glaffes, as they were fuppofed capable of fuch wonderful difcoveries: and that as well by candle-light, as (by what is recommended) the ftrongeft day-light: and yet he muft declare, with fome concern, that through the fmalleft globule, viz. of one half of a Paris point in diameter, he has not been able to diftinguifh any thing; and even through that which magnifies the leaft, he could never view any object with fatisfaction; though he applied the moft minute, and confequently the properest objects for thefe glaffes, viz. the globules of the blood, the farina of vegetables, the feeds of mufhrooms, the feathers of butterflies, pepper-water, &c. He hopes his eyes are not injured by thefe examinations, as they have been much ufed to microfcopes; but he believes there are very few, who would not have been nearly blinded thereby.

Upon the whole,—Mr. Baker thinks the Royal Society much obliged to the Father di Torre for thefe fpecimens of his great dexterity, ingenuity, and patience, in forming and fetting glafs fpheres thus extreamly minute; but he confiders them as matters of curiofity rather than of real ufe.

Strand, April 17, 1766.

Henry Baker.

Received

Received December 1, 1765.

XII. *De Veneris Transitu, per discum Solis, A.* 1761, *d.* 6 Junii, *Auctore* F. Mallet, *Astronomo Regis* Upsal.

Read April 17, 1766.

QUUM Philosophicas Transactiones, A. 1763, nuper vidi, substiti inprimis circa duas ad parallaxin Solis pertinentes, quæ illam ex observato transitu Veneris per discum Solis determinandam docent; in iisdem vero, observationum Upsaliensium usum inveni minus accuratum, forte ex non sufficienti apud Anglorum astronomos cognitione circumstantiarum, in quibus factas easdem descripsi in Actis Stockholm, A. 1761. Quam ob caussam, illas denuo explicare & Societati Regiæ communicare decrevi, additis nonnullis meditationibus, quæ eandem materiam illustrare poterunt.

Anno 1761, 6 Junii, hora 2 ante meridiem, frequentes adfuimus in observatorio astronomico Upsaliensi, ortum Solis jam flagitantes, & singulare illud phænomenon transitus Veneris, per discum Solis, strenue exspectantes. Nostrum vero ardorem restinguebat subnubila cœli facies versus eandem plagam horizontis, quæ Solis apparitionem pollicebatur; nec minor fuit nostra anxietas, cum Solem, inter nubes, fluctuanter, per intervalla deteximus. Spes tamen supererat, aut descensuram esse nebulam, aut Solem

 e vaporibus

e vaporibus curſum directurum, ut copulæ gloriam cum rariore planeta mathematicorum adſtanti coronæ manifeſtaret. Augebat fiduciam Sol mox diſtinctius conſpectus in margine ſuperiori, & lumen ejus verſus inferiorem paullatim creſcens. Dubitavimus vero, adeſſetne Venus, ſuam præſentiam, plus quam ſeculo exoptatam, velociore ingreſſu accelera-tura, & ſi globoſæ quædam vaporum condenſationes in parte imaginis Solaris confuſe apparerent, quique a fallaci phænomeno, quaſi globulo viſo perculſus, dolebat, & cœli obſcuritatem querebatur, quæ raram Veneris in ſplendidiore Solis diſco immerſionem nobis obvelaret. Tandem inclaruit Sol paullo altior, ut eundem per integram faciem ſcrutari poſſemus, et Veneris abſentiam oculis armatis perſpicue ediſcere: hanc vero ut ſtatim haud injucunde tulimus, ita planetæ deſideratiſſimi acceſſum retardatum optavimus, quia Soli jam intenti hunc undique exæſtuantem vidimus per motum vaporum, quibus graviter involutus conſpiciebatur; erat videlicet limbus Solis vehementer quaſi ebulliens, ita ut ſollicitudinem haud levem injiceret marginum tremulatio de contactibus immerſionis rite non obſervandis. Solis præterea lumen admodum debile, quia horizonti vicinior, erat, adeo ut eundem abſque vitris coloratis per integra minuta intueri liceret.

Sub hæc, approprinquabat gratiſſima Venus; nobis autem de ephemeridum diſſenſu, in determinando primo ingreſſus momento, diſſerentibus, accidit, ut inopinato adeſſet hora 3, 20′, 45″, quod ego quidem primus obſervavi, poſtquam hora 3, 19′ circiter, margines Solis, examine inſtituto, intactas inveniſſem, & teleſcopium meum commodius ſub intervallo

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transſpoſuiſſem. Apparuit primo Venus hirſuta facie in meo teleſcopio, quod eſt 18 poll. Angl. a D. Short fabrefactum, & micrometro objectivo Dollondiano 30 ped. munitum, quodque ita adornatum objecta 55 vicibus magnifacit. Hujus vero inſtrumenti eam indolem deprehendi, quod objecta horizonti vicina haud probe terminata repræſentet, & in eodem caſu focum genuinum difficulter admodum aſſequi liceat; quapropter pluribus ante diebus, inſtrumentum hocce pleno ſuo apparatu inſtruxeram, & diverſa ratione focum exquiſiveram atque correxeram, notato indicis loco in ſcala, quæ ſpeculo minori adjuncta eſt, ne ſub tempore obſervationum de hoc dubium reſtaret. Eandem ob cauſſam, ſtatim poſt Solis ortum, indicem repetitis vicibus ſcrutatus ſum, & rite collocatum inveni; qua licet adhibita cautione, neſcio quomodo factum ſit, ut ſub contactu interiore immerſionis confuſio, licet maxime exigua, oculos moleſtaret, & paullo poſt focum alteratum deprehenderim: erat vero mutatio foci vix quingenteſima parte pollicis Angl. & obſcuritas marginum tantilla, ut aëris conſtitutioni, vel leviſſimæ oculorum ſub horis matutinis diverſitati a ſolita eorum acie tuto adſcriberetur; quibus præmonitis, debiti ordinis & perſpicuitatis gratia, ab obſervationibus Domini Strömer jam deſcribendis incipiam.

Hic aſtronomiæ ad noſtram academiam profeſſoriam emeritus, tum vero officio Carolicoronenſi defunctus, ad nos redux utebatur tubo refractionis 20 ped. Suec. cum oculari 0. 35 ped. quem in obſervandis immerſionibus & emerſionibus ſatellitum Jovialium per viginti annos adhibuit; interpoſuit vero vitrum rubrum in obſervationibus Solaribus ab illo ſemper adplicatum. Ab hoc aſtronomo accuratiſſimo & quam maxime

exercitato,

exercitato, contactus immersionis Venereæ interior hora 3, 38′, 5″ æstimatus fuit; ordo vero observationum hic erat: hora 3, 15′ Solem e nube, qua involutus fuerat, emersum vidit, sed Veneris adventum non nisi hora 3, 42′ circiter exspectabat. Eodem vero cognito h. 3,21′, Solem Veneremque intuitus, utriusque margines male terminatas perspexit. Hora 3, 24′,35″ margo Solis ad illas Veneris perpendicularis videbatur, sed continuavit simili apparentia tempore 2 aut 3 minutorum, adeo ut verum momentum dimidii ingressus valde incertum esset. Dein appropinquante planeta ad contactum interiorem, Venus quidem circularis apparuit, sed figuram excedens versus marginem Solis, eodem fere phænomeno, ac guttula aquæ corpori adhæret, & hoc eadem ex parte magis extensum præbet. Eadem excrescentia Veneris parva Solis cornua distinguebat, quæ subrotunda erant; Venus quoque largius supra marginem Solis extensa, quam par credebatur, huic adhærens, immersionem tamen intra pauca momenta perficiendam ostendebat. Mutabatur paullatim singulare hoc Veneris incrementum, ex quo tarda videbatur Venus in margine Solis linquendo, aut foribus ingressa post se claudendis; Sol contra, sub oscillatione cornuum, contactum interiorem festinare & planetam leviore vi a limine propellere, ut pleno amplexu sua Venere frueretur. Sub brevi hac pugna, planeta, toto licet corpore ingressus, ligamine quodam margini Solis unitus conspiciebatur; idem etiam distentum quasi, in medio ruptum fuit hora supra dicta 3, 38′, 5″, & spectatoribus Venerem intra Solem paulisper moraturam pollicebatur. Distantia Veneris, cujus imago jam distinctior, a margine Solis mox apparuit 8 circiter secundis, sive octava aut sexta

parte diametri suæ. Sunt hæc fere totidem verba nostri Strömeri in Diario Observatorii Upsaliensis, quæ jam adduxi, ut insigne hoc phænomenon uberius inculcarem, quia a nemine observatorum extra Upsaliam æque perspicue fuit descriptum, aut cum suis circumstantiis adeo exacte observatum.

Sub mora transitus Venerei ad filum verticale observavit D. Strömer appulsus marginum Veneris ac Solis & horizontale quadrantis 3 ped. Suec. quos omnes hic recensere nimis longum foret. Accedente vero Venere egressura, ad marginem Solis occidentalem, paullo ante contactum interiorem, vel prius quam erumperet Venus, splendor Solis ante illam, sive ea ex parte, ad quam procedebat planeta, offuscabatur, & pressa ab illo Solis margo, atque in filum extenuata, flavescere incepit, mox vero cum exiguo Veneris disco coincidere & evanescere hora 9, 28′, 0″. Adnotavit hoc momentum pro contactu interiore D. Strömer in schedula, quam pro describendis observationibus ad manus habebat, & mox Solem respexit: verum jam largiter excisa videbatur Solis a Venere obscurata margo hora 9, 28′, 7″ ut miraretur noster tantam in illa a parvo planeta & tam brevi tempore factam aperturam. Illi præterea singulare hoc visum fuit, admodum obtusa apparere exigua Solis cornua, ut judicari posset, Venerem intra discum Solis adhuc restare, licet ignota vi marginem ejus obtegeret, aut nostris oculis subduceret. Emergentem dein Venerem armato oculo persecutus, ejus centrum in margine Solis collocatum putavit hora 9, 35′ circiter, ut supra de immersione dictum fuit; contactum etiam exteriorem dubium accepit, mutato foco tubi ex perculsione instrumenti sub fulcri transpositione, quæ paullo ante contactum evenit, ita ut genu-

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inum focum reſtituere non potuerit ante $9^h, 46', 13''$, quo Venerem Soli valedixiſſe dubitans credidit.

Aderat Dominus Melander, mechanices tum director, nunc Strömeri ſucceſſor, & aſtronomiam multo cum applauſu ſcienterque profitens: illi vero tubum 16 ped. Suec. conceſſi cum vitris rubris, tenui & non polito pro immerſione, craſſiori vero et perobſcuro pro emerſione Veneris, quia omnem defectum vitrorum coloratorum antea ſupplere non licuerat. Quanta fuit acies & perſpicacia hujus obſervatoris ex obſervationibus patebit; nam hora $3, 37', 9''$ Venerem intra diſcum Solis contineri judicavit, ita ut circuli diſcorum ſe invicem tangere viderentur, licet obſcurata maneret exigua pars marginis Solaris, ſuper quam nuper processerat Venus: hora $3, 38', 2''$ vero contactum interiorem abſolutum vidit. Hæc ex aſſe conveniunt cum obſervatione Domini Wargentin, niſi quod intervallum eorundem phænomenorum ab illo ſtatuatur tribus ſecundis majus. Paulſo ante contactum interiorem emerſionis, defuit cochlea fulcri, quod tubum Melandri ſuſtentabat, ita ut eundem manibus levare coactus fuerit, quod ipſe infortunium ſagaci prudentia pratienter tulit, ne ſuas collegas turbaret, ſub inſtante e Sole egreſſu; ipſe vero oculis contactum exteriorem inſtantaneum determinavit hora $9, 46', 29''$.

Sequitur jam, ut in propriis obſervationibus recenſendis pergam. Primo itaque undulationes marginis Solaris circum planetam tolli penitus animadverti, & extendi excavationem illius fere in lineam rectam; deinde intra pauca ſecunda dilatari obſurationem limbi Solaris ultra modum, quem ferebat magnitudo Veneris poſtea viſa; & quemadmodum parva hæc Solis eclipſis poſt contactum exteriorem immerſionis, quem

quem tamen videre non licuit, ſubito creſcebat, ita verſus contactum interiorem rationem figuræ Veneris non imitabatur, ut ſupra explicatum eſt in obſervationibus Dom. Strömer. Erat quoque hora 3, 27′, 12″, cum Venus ultra dimidium immerſa certo apparebat, licet per contactum interiorem evidens ſit, centrum ejus ante horam 3, 28′, marginem Solis non attigiſſe. Poſtea vero cornua Solis extra circulum diſci circum Venerem pauliſper ſurgentia videbantur, & mediante lumine fuſco, quod Venerem circumdabat, integra hæc apparuit 4 circiter minutis poſt momentum medii ingreſſus, vel forte antea ſi advertiſſem; imago tamen partis a Sole averſæ eo confuſior quo magis a Sole diſtabat. Videbatur præterea Sol, ſub vehemente limbi ſui æſtu, niſum exercere ad planetam complectendum, eo majorem, quo magis tendebat Venus ad contactum interiorem: ante hunc, porrexit ſua cornua Sol extra circulum diſci, diſcrimine vere notabili, verſus horam 3, 37′, 47″; poſt hoc vero momentum, circa Venerem oriebatur levior lux, primo confuſa, deinde creſcens in diſtinctiorem, ita ut hora 3, 37′, 56″ cornuum Solarium conjunctio eſſet ſenſibilis; verum radii pone Venerem emicantes, ſub debili adhuc Solis lumine, difficulter admodum obſervabantur, ita ut ſplendoris levitas dubium me redderet, utrum veram marginem Solis diutius exſpectarem, donec intra ſemi-minutum Venerem ſuper Solem progreſſam, hujuſque marginem clariorem poſt illam conſpicerem. Equidem non diffiteor, novam hanc apparentiam me aliquantiſper ſuſpenſum tenuiſſe, id tamen audeo aſſeverare, contactum interiorem fuiſſe perfectum dicta hora 3, 37′, 56″; neque, licet de indole phænomeni præmonitus fuiſſem, eundem ante 37′, 52″ non potuiſſe

potuisse assignare. Advenerant paucis ante diebus suasiones Domini De Lisle, ut in observando transitu Veneris vitrum fumigatum cum viridescente conjungeremus, quam ob rem, accepto vitro rubro, loco fumigati, tenue ac viride illi super imposui, sed ob lumen Solis sub immersione admodum debile eundem apparatum tollere coactus sum, ut perspicacius viderem, & vitrum gracile virideque substituere, per quod Solis imaginem albescentem intuitus sum: eadem quoque fuit apparentia in descripto vitrorum apparatu, quem postea resumsi & constanter retinui.

Procedente Venere a contactu immersionis interiori, omnium primo ejus diametrum determinare studui, quam, mediante proportione mensurarum diametrorum Veneris & Solis, optime inveniri putavi. Diametrum igitur Veneris, quæ exigua erat, utrinque sumsi respectu initii scalæ micrometri Dollondiani, quod in meo telescopio has excursiones instituere sinit, eamque constanter & sub toto transitu, non majorem 51 partibus scalæ, nec minorem 50 inveni. Diametrum etiam reperi eandem in quacunque directione mensuræ institutæ, sive verticali ad horizontem, sive obliqua utcunque, sive horizontali, adeo ut valde miratus sim Dominum De la Chappe diversas ejus magnitudines in diverso Veneris situ supposuisse: certe variantem ejusdem mensuram invenire mihi quoque contigit fatigato oculo; sed, & vigore restituto, & debita attentione adhibita, limites jam indicatos in dicto telescopio genuinos fuisse fidenter assero, & aciem oculorum vix intra pertingere nisi conjectura. Duplicem vero hic proposui limitem, quia de maximo & minimo illius determinando sollicite laboravi, ambas Veneris imagines vel tantillum ad invicem retrahendo, vel ad arctiorem contactum perducendo.

cendo. Simili ſtudio, limites diametri Solaris quæſivi; verum margines Solis, ut & aliorum corporum lucentium, vix eadem accuratione aſſequi licet; & quia minor in his jam requiritur exactitudo, limites inventas eo uſque extendam ac fuit differentia obſervationum maxima, ſcil. diameter horizontalis Solis minima at rarius accepta fuit 1665 partium ſcalæ, maxima 1671, quam veræ propiorem eſſe plures obſervationes confirmant. Poſita itaque diametro Solis $=31', 35''.5$ patet illam Veneris non fuiſſe minorem $56''.7$ nec majorem $58''.1$, adeoque mediam pro apparente ſub tranſitu ejus accipiendam $=57''.4$, cui apprime conveniunt fere omnes determinationes diametri Veneris.

Inter has obſervationes, quas pro diametro Veneris invenienda inſtitui, facile intelligitur, quod diſtantias Veneris a margine Solis inferiore minus neglexerim menſurare. Eas vero hic omnes recenſere nolo, quoniam ab aſtronomis uſum ejuſmodi obſervationum hucuſque factum nequaquam inveni, meiſque obſervatis diſtantiis calculum per otium adplicavi, quem brevi abſolvere conabor. Præterea exiſtimo, mediantibus hujuſmodi obſervationibus, ſatis tuto determinari parallaxin Solis ex unius loci obſervationibus; unde D. Maſon noſque Upſalienſes eandem parallaxin in poteſtate habere credo, per ingentem numerum diſtantiarum ab illo nobiſque captarum: ſi quis vero meas obſervationes communicatas deſideret, antequam ſupputationes dudum inceptas perfecerim, huic lubens adero, quia menſuratas diſtantias, ultra modum reliquarum obſervationum ad Venerem inſtitutarum, certas & indubias exiſtimo. Ex his autem, minimam centrorum diſtantiam a me ſumtam in præſenti ſolum explicabo.

explicabo. Accidit vero ut, circa tempus dimidiæ moræ Veneris in Sole, poſtquam per horæ octavam partem quieviſſem, aſſiduus in obſervando diſtantiam centrorum vix amplius variare deprehenderim; & quia de medietate tranſitus Venerei nulla apud me opinio adhuc ſurgebat, illud phænomenon primo obſervationum vitio tribuerem; deinde, omni adhibita cautione, eidem medelam adferre ſtuderem; ſed quo major opera & quo certior eſſem de bene inſtituta obſervatione, eo magis diſtantias coincidentes obſervaverim. Jam itaque perſuaſus de exactitudine menſurarum, obſervationes refraganter quidem deſcripſi, ſed fideliter tamen adnotavi, ſub anxia mentis dubitatione de cauſſa irregularitatis (licet præter naturam ſuppoſitæ) qualem in hoc inſtrumento antea non inveneram. Crevit etiam ſub paucis momentis animi deſpondentia, quando diſtantias marginum paullo diminutas mox obſervavi, unde de menſuris exactiſſime capiendis maxime ſolicitus tempori opportuno relinquere decrevi, ut de his rite diſquirerem. Illuſtravit vero omnia Venus a centro Solis magis diſcedens, animumque ſic reddidit timidulo ſuo obſervatori. Hæc vero cum ita evenerint, minor apud me manet ſuſpicio de quantitate minimæ diſtantiæ centrorum Veneris & Solis haud exacte determinata. Quod ad methodum attinet, qua diſtantias marginum aut centrorum erui, hæc talis fuit: marginem Veneris Soliſque in duplicata imagine ad contingentiam perduxi, & convolvendo micrometrum detexi ſpeciem hujus contactus, in inſtrumento videndam, ut diſtantia a margine acciperetur minima, h. e. ſita in linea recta, quæ per centra tranſit. Difficilior ab initio erat hæc opera, & parcior hinc numerus obſervationum, ſed

 paullo

paullo adſuetus uno fere ictu oculi perſpexi, utrum genuinus eſſet contactus marginum Veneris atque Solis, manſitque error ſemper intra partem ſcalæ quæ minutum ſecundum proxime continet. Intelligent hoc obſervatores, qui macularum Solarium diſtantias a margine Solis proxima, ſimili inſtrumento, ſumſerunt, mihique brevitatem jam ſtudenti concedent obſervationum certitudinem non uberius arguere, quia in ejusmodi menſuris rariſſime ultra minutum ſecundum aberrare ſoleo, modo debitam attentionem adhibuero. Hac igitur ratione accepi diſtantiam marginis Veneris tam ſuperioris quam inferioris, ſive proximæ & remotæ, adeoque ipſius centri diſtantiam a margine Solis proxima, unde illam centrorum ex determinatis Solis Veneriſque diametris facillime ſupputavi. Ad minimam vero centrorum diſtantiam ejuſdem limites ſollicite quæſivi, quos eo certiores exiſtimo, quo magis ex obſervationum exactitudine unice dependent, & opinioni conceptæ potius repugnantes quam accommodatæ fuerunt: ſcilicet habebatur eadem diſtantia æqualis 520 aut 521 partibus ſcalæ micrometri, ipſaque diameter Solis capta in eadem obliquitate ad horizontem ac diſtantia centrorum planetæ Soliſque fuit 1670 aut 1671 partium ejuſdem ſcalæ. Obſervandum hic eſt, obliquitatis diametri Solaris mentionem ſtudio hic factam, quia diametrorum Solis variationem in menſuris, pro diverſa illarum inclinatione ad horizontem, ſoleo deprehendere, præter illam quæ a refractione dependet: eadem vero adeo eſt exigua, ut pauca tantum comprehendens ſecunda dubium me ſemper reliquerit, cuinam cauſæ erit tribuenda, an veræ apparentiæ, vel inſtrumenti defectui, vel etiam incertæ oculorum hebetudini, quæ continuatis obſervationibus

Solaribus

Solaribus paullatim crescit, & irregularitatem mensurarum prodit. Quicquid sit, accepta diametro Solis $=31'$, $35''$, ut antea, per rationes limitum minorum erit distantia centrorum $=9'$, $50''.2$, per rationem vero limitum majorum $=9'$, $51''$, harumque determinationum media $=9'$, $50''.6$: omnes autem hic nominatæ mensuræ, ut certiores essent, in eodem loco campi telescopii, prout fieri protuit, sumtæ fuerunt ex rationibus supra descriptis. Quamobrem a Domino Pingré dissentire cogor, quando eandem distantiam $=9'$, $55''.6$, ex mora Upsaliensi transitus colligit, quia, ut patet, ex immediatis mensuris determinatur quinque secundis minor, & observationum diversimode institutarum plenum consensum, ad me totidem secundis decipiendum, per cognitam mihi exactitudinem, & circumspectionem, quibus peractæ fuerunt, denegare audeo: neque distantiam centrorum Veneris atque Solis hic stabilitam incongruam quis judicabit, modo consideret eandem ex utriusque marginis Veneris distantia a margine Solari deductam esse, unde errores mensurarum se invicem compensare necesse est, si qui ad imaginum contactum commissi fuerint. Præterea omnis fere labendi occasio, qua distantias marginum nimis magnas invenire debui, præventa fuit, eo studio, quod nuper enarravi: haud etiam credibile videtur, si vera distantia foret $=9'$ $55''.6$, me, in omnibus mensuris, circa minimam distantiam sumtis, a directione centrorum Solis & Veneris per $2°$, $23'$ constanter aberrasse, quod fieri oportuit, ut dictam centrorum distantiam quinque secundis vera minorem determinarem. Ex his itaque patet, parallaxin Solis, repugnantibus jam descriptis observationibus, a celeberrimo Pingré nimis augeri, & illam ex iisdem infra

M 2 novem

novem ſecunda ſtatuendam eſſe: quiſque enim intelligit, data ratione diametri Solaris ad diſtantiam centrorum minimam, hanc ex illa cognita tuto determinari, & ſi quæ obſuritas limbi Solaris viſioni noceat, debebit eadem, aut in utramque menſuram æqualiter influere, aut extimas Solis partes inviſibiles reddendo, Venerem ad marginem Solis proximam paullo retrahere, adeoque diſtantiam centrorum extendere; erit itaque diſtantia minima centrorum Veneris & Solis ſub hoc tranſitu $=9', 50''.6$ pro obſervatorio Upſalienſi, in aſſumpta Solis diametro, in illa vero a Pingré ſuppoſita $=9', 51''.2$, quod me parum moratur.

Progredior ad apparentias exitus Veneris e Sole, quæ omnino ſingulares mihi viſæ ſunt: ad has vero obſervandas præmonitus acceſſi, & ſimilia in emerſione phænomena exſpectavi ac ſub immerſione, ſed ſpe ex parte fruſtratus ſum; eo diligentius tamen ea quæ apparuere potui adnotare; videlicet hora 9, 27′, 30″ circiter, Venerem margini Solis multum approximatam vidi, & totam animi vim exercui ad momentum contactus interioris feliciter & exacte determinandum. Hora 9, 27′, 55″ circuli diſcorum Veneris atque Solis ſe invicem contingere videbantur, ſed Venus a Solis margine undique comprehendebatur, quia hæc ſupra Venerem elevata paullo apparuit. Eadem excreſcentia Solis intra pauca ſecunda extenuabatur, & paullatim minuto lumine ejuſdem, hora 9, 28′, 1″ marginem Solis adhuc quidem continuam oſtendebat, ſenſim vero evanuit, ita ut momentum præciſum vix determinari potuerit: tamen hora 9, 28′, 3″ cornua Solis adeo diſtincta apparuere, ut Venerem per ſaltum excurriſſe crederem. Figura Veneris circularis diſtinctior

ſtinctior quoque videbatur, a debili lumine, quod illam circumdabat, eadem ratione ac ante immerſionem, & obtuſa Solis cornua ultra circulum diſci exporrecta æſtimabantur. Lumen vero Venerem, poſt hunc contactum, comprehendens paullatim evanuit, in parte a Sole averſa, & adverſæ ſolum inhæſit, donec Venus fere tota emerſerat: diſparuit idem circa duo minuta ante contactum exteriorem, & Venere ad illum appropinquante, hæc rurſus, quaſi per lineam fere rectam, ſuper marginem Solis, largo contactu, extendebatur, quemadmodum de prima apparitione immerſionis dictum fuit; deinde rotundius apparuit fruſtum obſcuratum, tandemque in angulum deſiit, qui hora 9, 46′, 23″, major recto apparuit; poſtea idem angulus acuebatur, & ſpeciem mucronis gladii exhibuit, qui unico inſtanti e Sole excidit hora 9, 46′, 29″.

Hiſce omnibus jam recenſitis obſervationibus addi merentur illæ Domini Bergman, olim Matheſin ad noſtram academiam legentis, nunc munere adjuncti philoſophici eandem profitentis. Fuit vero tubus refractionis, quo hic utebatur, reliquorum omnium præſtantior, quia vitrum objectivum 21 ped. Suec. ab illuſtriſſimo Domino Klingenſtierna A. 1754 fabrefactum, oculare vero 0. 29 ped. a Lehnbergio noſtro nuperrime paratum erat. Idem inſtrumentum proprio uſui deſtinaveram, ſed multiplicatis præter meam opinionem obſervatoribus, eundem tubum Bergmanno conceſſi, ut foci alterationem in teleſcopio meo, quæ, ob diverſam oculorum conſtitutionem, neceſſaria erat, penitus evitarem. Eſt vero hic tubus diverſis vitris rubris inſtructus, quorum tenuiſſimum adhibuit D. Bergman ſub immerſione, craſſius autem ſub emerſione Veneris: evenit hinc ut D. Bergman, omnium

omnium præſentium optime armatus, contactum immerſionis interiorem, ſub minus favente cœlo, citius exploraverit quam reliqui adſtantes, nempe hora 3, 37′, 43″, pariterque contactum emerſionis interiorem paullo ſerius acceperit, quam a reliquis fuerit obſervatus, hora videlicet 9, 28′, 9″: erat tamen minor hic differentia temporum quam in ingreſſu, ob circumſtantias cœli, reſpectu apparatuum, magis coincidentes. Tandem contactum exteriorem obſervavit D. Bergman hora 9, 46′, 30″, eumque inſtantaneum judicavit.

Sequitur jam, ut nonnulla tranſitus Veneris phænomena repetam, & noſtras meditationes circa præſentem materiam adferam. Primo itaque, durante ingreſſu Veneris, pars ejus immerſa major ſemper adparuit, quam par erat: augebatur videlicet Solis diameter ab æſtuatione limbi per motum vaporum, ſed accedens planeta ejuſmodi diſci Solaris incrementum tollebat, quantum a Venere tegebatur margo Solis, unde etiam, quæ actu deprehenſa fuit, minor æſtuatio circa Venerem. 2. Lumen illud debile, quod planetam, ante immerſionem perfectam & poſt emerſionem inceptam, circumdedit, adſtipulantibus reliquis obſervatoribus Upſalienſibus, & ſuo ſuffragio adnuente Domino Wargentin, qui idem ſimul conſpexit, atmoſphæram Veneris indicare credo, quæ ſententia plena demonſtratione haud carere videtur, ſi omnia jam recenſita phænomena colligamus. Et ſi quis ſtatuat eundem ſplendorem ab atmoſphæra Solari derivandum eſſe, huic injunctum erit rationes explicare, cur in eclipſibus Solaribus a Luna perfectis deſit omnino eadem apparentia. 3. Ex allatis colligitur ordinem obſervationum contactuum interiorum vim vel efficaciam

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ciam inſtrumentorum & apparatuum ſequi juſta fere proportione, quantum ſerenitas cœli permiſerit, adeoque earum exactitudinem eſſe in aprico, prout hæc a noſtro officio & induſtria dependet; & niſi hæc obtigiſſet earum ſucceſſio, obſervationibus minor fides vel auctoritas tribuenda fuiſſet, quemadmodum de Grenovicenſibus idem merito docuit D. Short. 4. Denique patet, tutiſſimam fuiſſe obſervationem contactus exterioris emerſionis, ex coincidente momento plurium aſtronomorum nulla ratione inter ſe communicantium, ſed momenta contactus ſeparatim, licet in eodem loco, adnotantium, ex audito numero ſecundorum, quem ex horologio Grahamenſi, per vices ſed conſtanter, enunciabant ſtudioſi aſtronomiæ ad hoc munus diſpoſiti & exercitati. Idem confirmat harmonia obſervationum D. D. Klingenſtierna & Wargentin, qui Stockholmiæ fuerunt obſervatores in praxi aſtronomica maximè idonei, & optimis inſtrumentis inſtructi. Inſtituto igitur genuino examine obſervationum noſtrarum elucet, neutram alteri præferendam eſſe, niſi ex rationibus apparatus inſtrumenti obſervatoriſque in correſpondente obſervatione, quod ut evidentius fiat, hic monebo, differentiam meridianorum Upſalienſis & Stokholmienſis, ex multiplici correſpondentia obſervationum circa eclipſes ſatellitum Jovis, derivatam eſſe 1′, 42″, quam etiam confirmant obſervati contactus, interior immerſionis & exterior emerſionis, adeo ut hanc 1′, 59″ aſſumere non liceat, ut fecerunt D. D. Short & Hornſby. Præterea obſervandum, ephemerides D. De la Lande, quæ ſtatuunt differentiam meridianorum Upſalienſis & Pariſienſis eſſe 1^{h}, 1′, 30″ corrigendas eſſe, ita ut ponatur eadem 1^{h}, 1′, 9″ quando pro Stockholmia aſſignatur 1^{h}, 2′, 51″,

eandem

eandem differentiam deduci ex correſpondentibus obſervationibus eclipſium ſatellitum Jovis, ut in Actis Upſalienſibus hoc demonſtratum eſt ex communicatis dictis obſervationibus apud Pariſios inſtitutis. Huic vero differentiæ meridianorum Upſalienſis & Stokholmienſis, quam nuper dedi 1′, 42″, repugnat, quod ſcio, contactus interior emerſionis utrobique obſervatus, qui etiam D. Wargentin & me ipſum ſtatim ab initio anxios tenuit, quia tantam diſcrepantiam momentorum, quæ eſt circiter 20 ſecundorum, nulla ex certa cauſa deducere valemus; dedi tamen, & jam adducam conjecturam non levi fundamento innixam. Accidit Upſaliæ, quod probe memini, & in diario deſcripſi, ut paullo poſt eundem contactum, emerſionis nempe interiorem, nubes Solem obtegerent, deinde cito tranſirent, & Solem perfectæ claritati reſtituerent, antequam centrum Veneris in limbo Solis verſaretur: potuit ergo, opinor, fieri, ut aëris quædam intemperies adfuerit, quæ a nobis haud perſpecta fuerit, quemadmodum nubes appropinquantes præcedere ſolet tenuior nebula, quæ ab oculis obſervatorum interdum non percipitur, at in phænomenis eclipticis accelerandis vel retardandis maximam vim exercet, quia horum momenta citius vel ſerius, pro diverſa obſervantium acie, ſæpius adnotantur, & eorundem perſpicacia ab aëris cœlique ſerenitate dependet. Hæc confirmant plures comparationes immerſionum & emerſionum ſatellitum Jovis, Upſaliæ & Stockholmiæ obſervatarum; confirmant quoque aberrationes altitudinum correſpondentium Solis, ſi quando nubes leviores huic appropinquaverint. Sic igitur Sol tenuiore hoc velo, nobis inſciis, forte obductus, potuit contactum emerſionis interiorem aliquot ſecundis acceleratum oſtendere.

Provoco ad aſtronomorum experientias, ſi quid huic ſimile ex aëris conſtitutione derivandum probent, non aliter namque licebit diſſenſum momentorum explicare, quando obſervationes, ut apud nos factum, in nexu phænomenorum ſatis exacte conveniunt. Elucet quoque, accelerationem phænomenorum obſervationibus Upſalienſibus tribuendam eſſe, quia contactus exterior paullo poſt feliciter obſervatus cum illo Stokholmienſi exactiſſime adnotato plane coincidit.

Quemadmodum igitur ex allatis patet, neutram obſervationum Upſalienſium alteri præferendam eſſe, ita mea ſententia perſpicuum erit, nulli harum poſtponendas eſſe obſervationes Stokholmienſes, quod ex ſuperioribus a D. Short conceſſum iri credo, licet in ſua Tranſactione contactum interiorem immerſionis a D. Wargentin obſervatum, quaſi erroneum aſſumat, quod nequaquam probandum exiſtimo. Novi, & ipſe monſtravit, ſuam in hoc puncto ſagacitatem dictus D. Short; ſed noſtras e longinquo inſtitutas obſervationes eum haud probe cognoviſſe patet, quod in minori cum noſtris aſtronomis commercio ægre feret nemo. Cavendum præterea hic eſt, ne cum nonnullis illas inprimis obſervationes ſeligamus, quæ noſtræ ſententiæ de parallaxi Solis magis favent. Concedamus quoque obſervationibus hujus ordinis eam incertitudinem, quam inter plures ejuſdem loci obſervatores cum diverſis inſtrumentis regnare ſatis evictum eſt. Equidem ſcio moram tranſitus Veneris a me obſervatam minorem eſſe, quam quæ convenit diſtantiæ centrorum ope micrometri Dollondiani a me determinatæ; ſed unam vel alteram rejicere incongruum puto, niſi ex peractis diſquiſitionibus, (quarum inſignia exempla dedit D. Short) quantum officiant errores nunquam

 ſatis

ſatis evitandi pateat: ex illis enim juſto demum colligitur cuinam obſervationum generi major fides habenda ſit. Norunt aſtronomi accuratiores, & aperte docuit D. Bouguer in Eſſai d'Optique, quod margines Solis minus fulgeant, quam partes Solis centro propiores: forte hinc mirum non erit, margines Solis in contactibus interioribus diutius obſcurari, quam par eſt, & moram inter eoſdem nimium diminui pro ratione diſtantiæ centrorum Veneris & Solis, in cujus determinatione levior ex allata ratione metuendus eſt error, quod ex indole obſervationum intelligitur, & ex præcedentibus uberius colligitur. Taceo vires radios luminis reflectendi limitatas in utroque ſpeculo teleſcopii, nec non vitrorum tam objectivi in micrometro, quam ocularium & coloratorum in teleſcopio non perfectam pelluciditatem, ex quibus determinatur raritas vel denſitas radiorum a quovis puncto ad oculum pertingentium; hæc autem omnia in cenſum revocari poſſe ex eo probo, quod diametrum Solarem majorem ſoleo invenire, adhibitis vitris coloratis gracilioribus quam magis opacis; minorem quoque ſi levior intercedat nebula. Subſiſtit itaque irregularitas paucorum ſecundorum, quam ex obſervationum circumſtantiis rite explicatis unice dijudicare debemus: iiſdem vero conſideratis, patet obſervationem moræ tranſitus a me factam debuiſſe differre ab illa D. Wargentin, ſi utraque genuina fuerit, contra vero hujus obſervationes, excepto contactu interiore emerſionis, Upſaliæ obſervato, debuiſſe coincidere cum illa Bergmanni, ob inſtrumenta & apparatus proxime ſimilia.

Concludam, parallaxin Solis ex obſervata diſtantia minima centrorum Veneris atque Solis ſequi minorem

9 ſecun-

9 secundis, ut supra monui; sed nolle me hoc opus suscipere, ut litem inter astronomos ortam de vera Solis parallaxi, nisi post longum rationum examen, dijudicem: etenim observationes circa Martem institutæ annis 1750 & 1751 uno quasi ore clamitant, parallaxin Solis esse 9 sec. majorem, quod ex suis quoque observationibus monet D. Pingré. Contra vero observari merentur correctiones momentorum contactuum a D. Pingré, sine ullis adjectis rationibus, introductæ, ex quibus de illis controverti potest, nisi ex reliquis ejus observationibus per calculum evincatur, ultimo proposita momenta esse solum genuina. Favent quoque minori parallaxi Solis observationes contactuum a Masono peractæ, si cum Europæis fere omnibus comparentur, ut abunde demonstravit D. Planman professor physices Aboensis, in actis Stockh. 1762: restat vero, ut earum fidem ulterius urgeat D. Mason ex distantiis centrorum observatis, ne quid in opere calculi desit, quod ad observationum veritatem probandam facit. Perpendendum insuper credo, quousque extendatur certitudo observationum parallacticarum Martis, vel in quam partem pronior sit labendi occasio, quæ ex indole instrumentorum & methodorum observandi optime intelligi potest. His enim rationibus, ut arbitror, veram certamque Solis parallaxin ex jam factis observationibus colligere licebit, vel saltem earum irregularitati & certitudini medelam adferre in proximo futuro Veneris transitu A. 1769. Dabam Upsaliæ 28 Maii 1765.

Fredericus Mallet,
Astron. obs. reg. Ups.

Received Feb. 24, 1766.

XIII. *A Hepatitis, with unfavourable Symptoms, treated by* Robert Smith, *Surgeon at* Edinburgh, *now at* Leicefter.

Read April 24, 1766.

MRS Morton, aged 26, of a fpare habit and a fmall fize, in the fummer of the year 1750, frequently complained of a pain in her right fide. About the middle of July, the fame year, a violent vomiting and pain in her ftomach introduced an acute fever, accompanied with a conftant deep-feated pain in the right fide, under the coftæ nothæ, a little lower than where the ufual pleuritic fymptom in either fide is generally felt.

Copious bleedings, to fixty ounces and upwards, with a veficatory on the affected part, and pectoral medicines internally, afforded fome temporal relief.

July 27th, Upon the account of an intolerable pain at her ftomach, a very uncommon uneafinefs, and gripings in her belly, a medicine was exhibited, compofed of pulv. ftann. corralin. &c. and the day following, fhe voided by ftool a large annular worm, about half an inch broad, in length fix yards and a half.

Auguft 1ft, This day, upon examination, was difcovered a confiderable tumor, fufpected, from its fituation, and previous fymptoms, to be formed in the anterior part of the liver, of an oblong figure, and extended

extended its longeſt diameter acroſs the epigaſtrium about ſeven inches.

The patient, by this time, greatly debilitated by the large evacuations and fever, which ſtill exiſted, became ſo low and diſpirited, that ſhe had given over all thoughts of recovey, her huſband and relations being of the ſame opinion. To Dr. J. Dundas, an eminent phyſician, who had occaſionally attended, I propoſed making an inciſion into the tumor; though the event, uuder the preſent circumſtances, had but an indifferent aſpect. This propoſal was, however, approved of by the doctor, our patient, and her relations, under the following terms, viz. to have the opinion of the principal ſurgeon or ſurgeons in that city on the expediency of the operation, in order that, ſhould the experiment prove unſucceſsful, there might be no blame imputed afterwards.

Strong ſuppuratives, in the form of cataplaſms, were now uſed, whereby the tumor became more prominent in two days; a very deep fluctuation being felt, a large cauſtic was applied on the moſt depending poſterior ſide, thereby to avoid hurting the ſtomach or its appendages by an inciſion, which was made ſeveral hours after, from whence iſſued a copious diſcharge, at firſt purulent, at laſt glutinous, reſembling the white of an egg: no adheſion to the peritoneum could be felt, though accurately tryed all round with the finger.

Great attention and care were uſed in the proper applications, bandage, &c. particularly in the poſture of the patient; ivory and ſilver flat cannulas, kept in the aperture, were materially beneficial, as well for the conveyance of balſamic injections, as to facilitate the

the exit of the putrid contents. The 3d night after the operation, ſhe turned delirious; this ſymptom, with an increaſed fever and exceſſive cough, afforded little or no hopes of a recovery, the more eſpecially, as the diſcharge was now turned exceſſively thin, of a duſky colour and very fœtid: for theſe reaſons, I dreſſed her twice a day, throwing in large quantities of a warm injection, compoſed of a decoct. ficuum, and rad. alth. wherein was diſſolved balſ. capiv. to which was added, when the fever abated, ſome calomel ppt. In the mean time medicines internally, to allay her fever and cough, were not neglected; and ſhe afterwards took daily, as her ſtomach and other ſymptoms would admit, a light infuſion of cort. peruv.

By theſe means ſtrictly followed, about the 21ſt day from that of the inciſion, a laudable pus was obtained; but on the 23d, a thin ſanious diſcharge in great quantities burſt out, worſe than the former, and extremely fœtid.

Towards the end of the month, it began once more to aſſume a benign aſpect, but broke out a 3d and 4th time, on the 1ſt and 15th of September, every time the diſcharge growing more and more acrid, ſo as to excoriate and inflame the external parts; notwithſtanding theſe threatnings, by a cloſe perſeverance in the forementioned method, at the end of ten weeks, a callous cicatrix was obtained upon the external wound, and the recovery compleated ſoon after by the uſe of a few alterative mercurial pills.

The woman is ſtill alive, now (1766) in London, and enjoys a middling ſtate of health; only has been liable to complaints of gripes and indigeſtion, every

three,

three, four or five months. Her laſt complaint was generally relieved by a few ſaponaceous pills.

I am,

S I R,

Your obedient humble Servant,

Leiceſter, Feb. 19, 1766.

Robert Smith.

Received March, 1766.

XIV. *Experiments on the* Peruvian *Bark, by* Arthur Lee, *M. D.*

Read May 1, 1766.

THE great object of experiments is to eſtabliſh principles, on which practice may be conducted in the moſt expeditious and unerring manner. The intention, therefore, with which the following experiments on the Peruvian bark were made, was to confirm the pharmaceutic treatment of this medicine where it was juſt, to correct it where it was erroneous, or to improve it where it was defective.

Experiment I.

In the firſt experiment, I infuſed two ounces of the powder of Peruvian bark, in a pound and a half of diſtilled water; after 24 hours, it was filtered, and the filtered

filtered liquor ſubjected to diſtillation in B. M. till about half came over, which was limpid and of a diſagreeable ſlightly aromatic flavour: what remained in the retort depoſited, on cooling, half a dram of a reſinous ſubſtance, perfectly diſſoluble in rectified ſpirit of wine: the liquor being then evaporated in B. M. left about three drams of a gummy ſubſtance intenſely bitter and acerb.

Ob. It ſeems to appear from this, that the aqueous infuſion contains the aromatic part of the bark, with a little of the reſinous and a conſiderable quantity of the gummy ſubſtance.

Experiment II.

The reſiduum of bark, from the former experiment, after extraction for two hours, in B. M. with eight ounces of rectified ſpirit of wine, produced a tincture, which, when decompoſed with water, left three drams and eighteen grains and a half of reſinous matter. The water that was filtered from it was ſlightly bitter.

Ob. The bark therefore ſeems to contain three ſoluble parts, namely, the aromatic part, to be extracted by cold watery infuſion, a gummy part, chiefly diſſoluble in water, and a reſinous part diſſolved plentifully by ſpirit of wine. The quantity of diſſolved matter obtained in the above experiments is in much greater proportion than in the experiments of Mr. Boulduc and Mr. Neumann. Dr. Lewis obſerves, in his notes upon Neumann, that different ſorts of Peruvian bark differ conſiderably in their yield of extract, which, together with the heat uſed in making the ſpiritous extract, may account for this diſparity

in

in my experiments and thofe of the gentlemen juft mentioned.

Experiment III.

Two drams of a tincture obtained from one ounce of powdered Peruvian bark, with four ounces of rectified fpirit, and filtered without preffure, gave two grains of refinous matter; but the fame, when the filtration was finifhed, with preffure on the top, gave fix grains of the fame fubftance.

Ob. It may therefore feem advifable to ufe preffure always in filtering this tincture; and perhaps all the other tinctures would be rendered ftronger by the fame means.

Experiment IV.

Two drams of the powder of the external lamina of the bark, digefted in the cold with rectified fpirit of wine, afforded two grains of refinous matter.

Experiment V.

Two drams of the internal lamina, treated as above, gave a grain and half of refinous matter.

Ob. Though this fingle trial cannot determine the comparative quantity of refinous matter in each lamina; yet it may fhew, that each of them contains it, which, as far as I know, has not yet been proved by experiment.

Experiment VI.

Two tinctures were prepared from the fame quantities of the fame bark, and of the fame rectified fpirit of wine; but the one was digefted two hours, in B. M. the other 24 hours in the cold: a dram of the former yielded three grains, and the fame quantity of the latter one grain of refinous fubftance.

Ob. Were this experiment sufficiently authenticated, it would eſtabliſh a great preference in favour of the method of extracting tinctures in B. M. not only from its producing them more ſtrong, but from the much greater expedition with which the proceſs is executed.

Experiment VII.

Half a dram of the powder of bark was infuſed for 24 hours in cauſtic volatile alkali; and the tincture produced did not effervesce with acids, but retained a volatile-alkaline ſmell.

Ob. Is it therefore probable that the cauſtic volatile-alkali does not attract fixable air from the bark?

Experiment VIII.

When ſome of an aqueous infuſion of the bark was poured to ſome cauſtic volatile-alkali; a white cloud appeared at the bottom of the phial, which ſoon diſappeared, and the liquor became of a high reddiſh colour, without ſhewing any effervescence with acids.

Experiment IX.

Lime water and powdered bark, being macerated in the proportion of ten ounces of the former to one of the latter, in a well ſtopped phial, and then filtered; neither affected the colour of violet paper, nor precipitated the corroſive ſublimate from its ſolution in water.

Ob. This experiment was made a year before Mr. McBride had publiſhed his very ingenious eſſay on the diſſolving power of quick lime. The view with which it was made, was to determine, whether, after infuſion,

infusion, the lime water remained unaltered in its alkaline properties. And though I then conceived the reduction of the quick-lime to arise from the attraction of fixable air, yet it was by no means with that clearness and certainty, which arise from Mr. McBride's experiments. This experiment was repeated several times, with the same event.

Experiment X.

Lime water being poured to a watery infusion of the bark, gave it a high colour, and they remained pellucid; they tinged violet, green; and were distinctly alcaline and bitter. These things were observable for some hours after the mixing them.

Ob. This experiment seems to shew, that the air of the bark in substance, which reduces the quick-lime, does not enter into the infusion, or else adheres to it so firmly, that it cannot be similarly attracted by the quick-lime.

Experiment XI.

A cold infusion in common water produced no change on the syrup of violets.

Ob. I have somewhere seen it asserted, that the infusion turned violet, green; from whence an alkaline quality was inferred; to examine the truth of which, this experiment was made.

Experiment XII.

Half an ounce of powdered Peruvian bark was infused for 24 hours, in six ounces of a solution of sal ammoniac; when the menstruum had received a slight colour, but still remained clear and saline as before.

Experiment XIII.

Six ounces of a ſolution of common ſalt, infuſed for 24 hours, with half an ounce of the powder of bark, received from it a deep red colour, but retained its ſaltiſh taſte.

Ob. It was intended, that the two above experiments ſhould ſhew, whether the ſalt was precipitated from the water, during the ſolution of the bark.

Experiment XIV.

A well-ſaturated tincture of bark, with rectified ſpirit of wine, being added to lime water in the proportion of one third, ſuffered an immediate decompoſition of its reſin, as by common water; and in an hour, it made no change on the colour of violet paper.

I was in doubt here, whether the decompoſition might not ariſe from an affinity between lime water and ſpirit of wine, as in common water; to determine which, I made the following experiment.

Experiment XV.

The pure reſinous ſubſtance of the bark, put into lime water, was immediately diſſolved, and the lime water reduced.

Ob. There ſeems therefore to have been a double elective attraction in the fourteenth Experiment. That is, the quick-lime, attracting fixable air, was reduced; and the water, uniting with the ſpirit, the reſin was precipitated. Yet it is an objection to this ſuppoſition, that Mr. McBride's experiments prove the ſolution of reſins to be attended with the loſs of their fixable air.

Experiment XVI.

Vitriolic acid dropped into the ſolution (Exp: 15.) precipitated its reſin.

Experiment XVII.

Common water dropped into the ſolution (Exp. 15.) united with it uniformly.

Experiment XVIII.

Five grains of the reſinous part were agitated with one ounce of water, and one grain was diſſolved.

Experiment XIX.

Five grains of the reſinous part, being rubbed with an equal quantity of freſh quick-lime, and agitated with an ounce of water, were all diſſolved to one grain.

Ob. The very ingenious diſcovery of Mr. M^{c}Bride, regarding the abſtraction of fixable air in the ſolution of reſinous bodies, ſufficiently explains theſe experiments.

Experiment XX.

A dram of bark was infuſed for 24 hours in an ounce and an half of ſpirit of wine, then filtered, and the tincture decompoſed with water, and again filtered, ſo as to leave the reſinous part in the filtre; the gummy part which paſſed the filtre, turned a ſolution of green vitriol, black. The reſinous part was agitated with water, to purify it from any adhering gum; and what paſſed the filtre a ſecond time, gave a very light tinge of black to the ſolution of vitriol.

Experiment XXI.

The reſin, obtained as above, was rubbed with quick-lime, and then diſſolved in water, which ſolution, when

when filtered, gave a manifeſt black tinge to the ſolution of vitriol.

Exreriment XXII.

The officinal decoction, when cold, gives an evident black colour to the ſolution of vitriol.

Ob. Dr. Lewis obſerves, that the decoctions of the bark affect the ſolution of vitriol in a much ſlighter manner than the cold infuſion; from whence he infers, that the latter is more fully impregnated with the vegetable gummy matter than the former. I cannot ſay that I have obſerved any difference in the colour they ſtrike with the ſolution of vitriol, and am inclined to think, the reſinous as well as the gummy part poſſeſſes this power.

Experiment XXIII.

The matter, which had ſubſided from an officinal decoction, and which appeared to be purely reſinous, diſſolved in rectified ſpirit of wine, changed the ſolution of vitriol to a black colour.

Ob. The ſpirit of wine alone produced no change in the ſolution of vitriol.

Experiment XXIV.

Three parcels of bark, of half an ounce each, were infuſed in equal quantities of the proof ſpirit of the ſhops; after 24 hours one portion was filtered; another after 48 hours; and the third after 72 hours, or three days. The ſame quantity of each filtered liquor was united with equal quantities of the ſolution of vitriol; and after the decompoſed matter had perfectly ſubſided, the united liquors were each paſſed through three filtres of the ſame weight, which, when dried, were weighed

weighed again, and found to be perfectly equal, each having gained six grains of additional weight.

Ob. The common practice of prolonging the infusion to three days at least, is always tedious, frequently inconvenient, and, if we may trust this experiment, not necessary, because not useful, since the menstruum appears to have been as fully impregnated after one day's infusion, as after three.

Experiment XXV.

Three parcels of the same powdered bark were put, with six ounces of water to each, to infusion at the same time. After twelve hours the first was filtered, the second after twenty four, and the third after six and thirty hours infusion. The filtered liquors were not discernibly different in colour and taste, nor in a residuum obtained from each, as in Experiment 24, was there any perceptible inequality on the scale.

Ob. This experiment may direct our practice in preparing the cold watery infusion with more precision than is commonly known. I apprehend too, that it may further serve to amend our pharmaceutical practice in many other similar points, in which our veneration for the antients has induced us to acquiesce in their forms, which they did not found upon experiment, the only admissible test of their propriety.

These are all the pharmaceutic experiments I have hitherto made on the Peruvian bark; they were intended as a part of a compleat history of this medicine, which, though almost finished, an unexpected and indispensable call into my own country, prevents me from making public. I will just beg leave to subjoin a remark, concerning the tincture of the bark

with

with rectified ſpirit of wine, prepared by heat. I found the filtered water, made uſe of to precipitate the reſin, ſo ſtrongly impregnated, as to be more intenſely bitter than the watery infuſions; from whence I conclude, that ſpirit diſſolves not only the reſinous, but the gummy part, more powerfully than water; and as it is a more expeditious way than common decoction or infuſion, it might be more eligible for preparing the officinal extract. I have remarked too, that, after one ſuch extraction, the remaining bark is almoſt wholly inſipid, which ſhows how great the extracting power of ſpirit is, when aided by heat. In making this tincture, it is neceſſary that the ſtopper be taken out of the phial, a little after it has been in the heat, to let the extricated air eſcape, ſo that it may afterwards continue ſtopped without any danger.

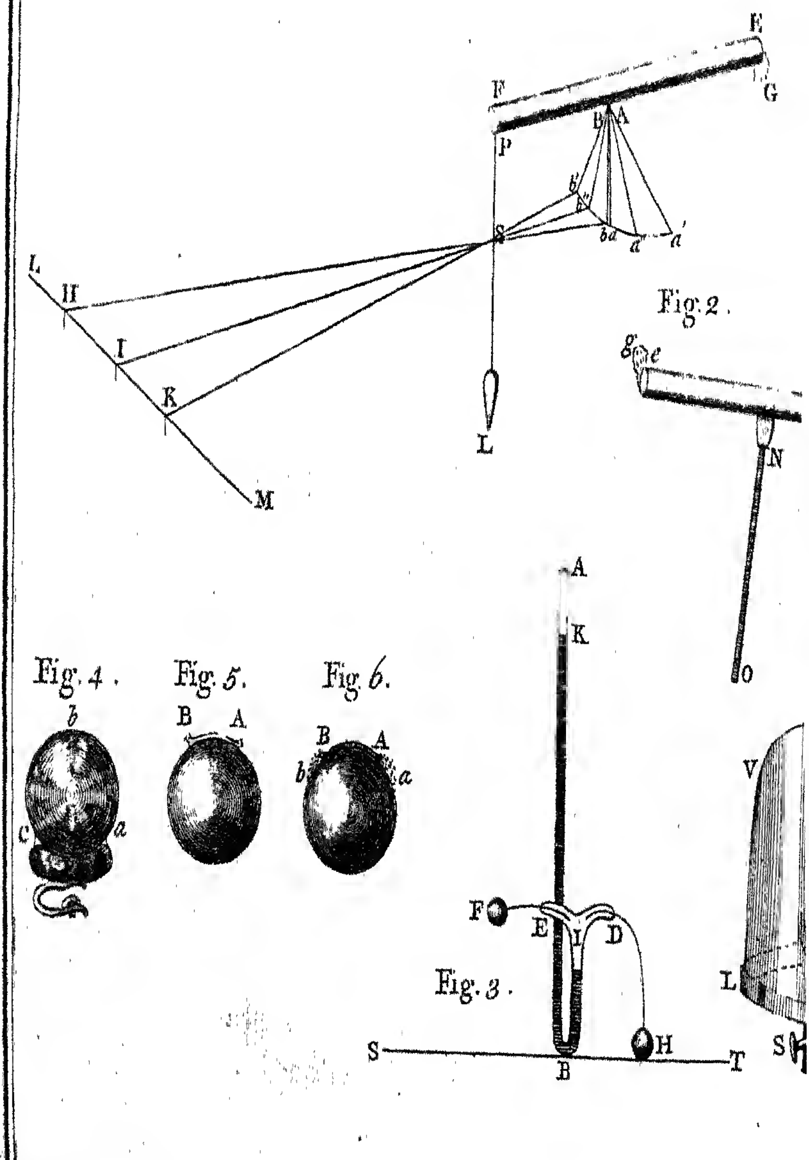
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Fig: 7.

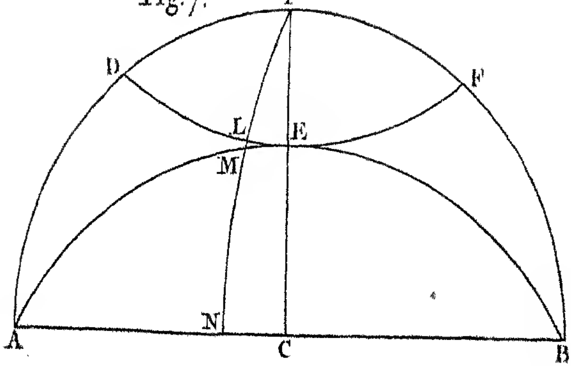


Fig. 8.

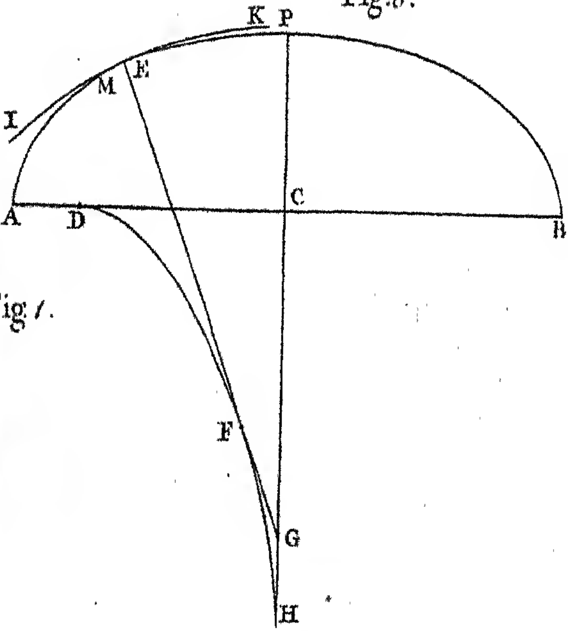
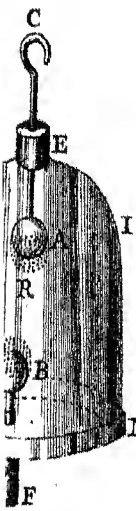


Fig 1.



J. Mynde Sculp.

XV. *Novorum quorumdam in re electrica experimentorum Specimen, quod Regiæ* Londinensi *Societati mittebat die* 14 Januarii, *Anni* 1766. Joannes Baptista Beccaria *ex Scholis Piis, & R. S. Soc.*

Experimentum primum.

Read May 1, 1766.

NEGO ceram signatoriam, sulphur, dum fricantur, orbari semper electrico igne suo. Vultis in hæc corpora ignem congeram alienum? Frico charta inaurata. Equidem ut igne exuam suo, satis est, si chartam invertam, & facie fricem nuda; sed ideo plane eloquentius videtur mihi experimentum; demonstrat enim a tantula superficiei crassitie pendere electricitatis contrarietatem, quantulam facit bracteola metallica.

Quod attinet ad contrarietatis veritatem, hæc experior. Quum alternatim utor facie inaurata, & nuda, penicillus & stellula mutant loca sua omnia in machina, in catena, in globo ipso, sive ex sulphure is constet, sive ex cera signatoria, &, quæ discesserunt, fricando una facie chartæ, stamina lini catenæ nexa, vel machinæ, accedunt, fricando facie ejusdem chartæ altera *.

* Quum hoc specimen optime merito de re electrica socio mitterem; ceræ signatoriæ, scribebam, annulum valde crassum advolutum cilindro ligneo, dum hic rotatur in machina electrica, cultro rotundo expolio, abrado, ceu faciunt tornatores, & ea ipsa in abrasione catena fit excessu electrica, machinæ defectu; si facie frico

Si frico ſimul globos ex cera ſignatoria duos eidem nexos catenæ, alterum nuda charta, alterum inaurata, perit electricitas altera vi alterius contraria: neque aliam a Frankliniana rei cauſam confingi oportet; ſcilicet quantum ignem immittit in catenam globus is, qui fricatur chartæ facie inaurata, tantum haurit alius, qui fricatur nuda.

Etenim Roberti Simmerii conjectationes de potentiis geminis, quas apellat antagoniſtas, miror ego, non moror. Profecto nihilo res adverſatur maxime, exceſſus defectui. Exceſſus elaſticitatis aërem movet; quidni ſimilis cauſa (quæ eſt Frankliniana Theoria) moveat ignem electricum? Una autem directione, quod rem conficit, moveri ignem electricum demonſtratur

Experimento altero.

Tab. V. Fig. 1. In vitrum pneumaticum VF admodum capax, per collum E, init virga metallica CA, deſinens in ſphæram metallicam A pollicarem politiſſimam. Sphæræ huic occurrit, ad aptum quoddam intervallum, æqualis ſphæra B, exiſtens ex lance machinæ pneumaticæ LN. Subducto aëre ex vitro, converſoque epiſtomio S, disjungo a machina pneumatica lancem

nuda, electricitates eunt in contrarias; ſi facie frico ejuſdem chartæ inauratæ, iterum catena exceſſu, machina defectu electricæ fiunt, & ſic deinceps. Cl. Wilſonus reſpondet experimentum ſe dudum antea cepiſſe, quo ceram ſignatoriam dudum per aliquot horas intactam fecit electricam defectu, ſemel ipſam fricando laevi argenti manu, deinde eandem iterum tranquillam per horas totidem fecit electricam exceſſu, fricando iterum ſemel, & ſimiliter blanda preſſione lævi eadem argenti facie, ſed paululum inclinata, ut ipſius limes ceram premeret. Ad ſulphur quod attinet, globo experiebar, in quo ſulphuri admixtum fuerat colophonium, ne facile fatiſceret.

cum

cum vitro; prehendo manu lancem LN, tum uncum C virgæ AC ineuntis per collum vitri admoveo catenæ (pono hanc fieri electricam frictione vitri) atque circa imum hemisphærium A virgæ ejusdem micat atmosphæra electrica, qua nihil in re electrica pulchrius vidi, aut ordinatius. Etenim tenui quidem ipsa est luce, luce tamen distinguitur usque & usque vividiore versus imum suum locum R. Forma hemisphærium lucis diceres majore diametro, existens ex imo sphæræ metallicæ hemisphærio. Interea vero circa sphæram B, quæ ad lancem pertinet, & cum solo communicat, atmosphæra nulla, nulla omnino deprehenditur lux. Pergo ad machinam constanter electricam, admoveo ipsi similiter uncum C; simillima atmosphæra illucescit circa sphæram B; circa sphæram A, lux cernitur omnino nulla. Generatim (uti expertus sum) quæ sphæra communicat cum corpore, in quo cum Franklino electricum ignem densiorem dicimus, atmosphæram habet electricam; quæ cum corpore, in quo dicimus ignem rariorem, habet nullam.

Monebo tamen aliqua in hoc experimento opus esse patientia, solertiaque; nam pro magnitudine electricitatis, & pro accuratione vacui, augendum intervallum sphærarum A, B, ne electricus ignis coeat in radium, eaque forma prosiliat a sphæra ad sphæram, uti in aëre admodum raro plerumque contingit. Sed sagax quisque naturæ vestigator semihora vix indigebit, quo faciat sibi satis. Quam experiendi pãtientiam videtur non demereri pulchrum hoc ac plane eloquens experimentum.

Etenim ipso demum oculis cernitur, qua ex parte ignis electricus proxime erumpat, unde fides fit maxima duobus, quos a principio, & primus usurpavi,

pro directione ignis electrici testibus, penicillo, & stellulæ. Scilicet constitui penicillum igne fieri erumpente, stellulam ineunte: ac plane penicillus erumpit ex iis corporibus, queis contigua sphæra metallica atmosphæra distinguitur electrica, stellula in iis corporibus fulget, queis contigua sphæra atmosphæra distinguitur nulla.

Pulcherrima itaque Roberti Simmerii experimenta circa serica tibialia huc redeunt omnia. Mutua frictione, tibiale album haurit a nigro ignem electricum nativum, qui est omnino plurimus. Intumescit seorsim tibiale utrumque, quod corpora æque electrica discedant, & partes singulæ tibialis unius sint æque electricæ; nam æque carent igne suo partes nigri, & partes albi æque redundant alieno; admota accedunt, quod accedant corpora inæqualiter electrica. Sed hæ sunt leges phænomenorum. Audebo alias experimentorum complexionem proponere, quæ causam jam plane prodere videtur discessionum, accessionum, cohæsionum omnino mechanicam.

Noletus, in suis ad experimenta Simmerii animadversionibus: Tæniæ, inquit, sericæ albæ vitro fricatæ ipsi adhærescunt, divulsæ ad ipsas redeunt præcipites. Hinc legem refellit, qua constitutum, corpora eadem prædicta electricitate se mutuo repellere. Sed, quod affert experimentum, confirmandæ erat aptius huic legi, quam convellendæ; nam tæniæ sericæ albæ exuuntur a vitro igne suo, quem adeo accipit vitrum. Quod tænia alba a nigra ignem accipiat, non sit inde, ut accipiat etiam a vitro. His fallaciis amovendis, & amplificandæ theoriæ electricæ universæ, atque novæ corporum omnium affectioni vestigandæ, utilis mihi videtur sequens tabula.

Effecta

Effecta experimentorum, quibus veſtigatum eſt, utrum duorum corporum, quæ mutuo fricantur, accipiat ignem electricum alterius, utrum det alteri ignem electricum ſuum.

Corpora aptata in machina electrica, rotata, & fricata ſimiliter ac aptatur, rotatur, & fricatur vitrum.		*Corpora, quibus fricavi, ceu communiter fricatur vitrum manu ac pulvillo.*
Vitrum lamellare aut ſolidum	politum ignem accipit	a corio inaurato, a charta inaurata, a manu *plurimum*; a corio nudo *multum*; a capillis, a tibiali albo, a charta nuda, a tibiali nigro *modicum*.
	aſperum dat	manui (uti iſtic inventum) pilis leporis, felis, martis, muſtelae.
Pili leporis	accipiunt	a corio inaurato, a tibiali nigro, a vitro aſpero *plurimum*, a numiſmate aureo, a numiſmate argenteo, a charta obducta bracteolis aureis, argenteis, aeneis, aut ſtanneis, a lamina oricalcica, aut ſtannea, aut ferrea, aut plumbea, a magnete, a regula ex fago, a tibiali albo, a taenia ſerica caerulea, a corio nudo, a manu, a charta nuda, a vitro aſpero *pauculum*.
	dant	vitro laevi lamellari, pilis felis, ſive albi, ſive nigri ſint, pilis aliis ſubtilioribus, vitro laevi ſolido.

Tibiale sericum	album	accipit	a tibiali nigro (quod est experimentum Simmerii) a numismate aureo, a lamella laevi aurea, a numismate argenteo, a lamina argenteo laevi, a panno nigro, a corio inaurato, a charta bracteolis aeneis, aurcis, argenteis, stanneis obducta, a lamina stannea, a vitro aspero, a panno serico villoso nigroquc, a theca Sandaraca obducta, vulgo *verniciata.*
		dat	chartae nudae, manui, capillis, pilis felis, leporis, mustelae, vitro laevi, panno serico villoso, sed albo.
	nigrum	dat	tibiali albo, panno serico villoso albo, pilis felis, capillis, pilis leporis, mustelae, martis, tubo vitreo laevi, magneti, oricalco, argento, ferro, manui, panno nigro ex lana.
Cera signatoria, & Sulphur		accipiunt	a corio inaurato, a charta aureis, argenteis, stanneis, aeneis bracteolis obducta.
		dant	capillis, pilis felis, mustelae, martis, manui, corio nudo, chartae nudae, tibiali nigro.

Expono paucis in uno, vel altero horum corporum, qua ratione expertus sim in omnibus. Tibiale nigrum cilindro vitreo advolvo, tendo, alligo, adsuo; apto in machina, ita dum rotatur tibiale nigrum, frico manu immissa in tibiale album; atque tum filum metallicum exhibitum tibiali nigro vibrat penicillum, effundit similiter penicillum filum metallicum nexum machinæ, aut exhibitum catenæ. Contra filum metallicum exhibitum machinæ, aut nexum catenæ demonstrat stellulam.

Sufficio

Sufficio cilindrum vitreum convolutum tibiali albo, frico nigro; ſtellula invadit loca penicilli, penicillus ſtellullæ.

Sed quoniam tibialia, & pelles cilindro vitreo, aut ſulphureo advolutæ corporibus quibuſdam nequeunt aptiſſime fricari, hinc experior etiam aliter. Experimentum pono. Famulus diſtendit manu utraque pellem felis calentem abs igne, ne ullus præterea inhæreſcat humor; tubo vitreo ego frico, mox aſpera ipſius parte, mox lævi; cum frico parte aſpera, filum ferreum exhibitum pelli diſtinguitur ſtellula, at exhibitum vitro effundit penicillum. Effundit, inquam; repente enim, & ſingulari cum crepitu atque expanſione explodit. Quæ adeo manifeſta effuſionis indicia non vidi alibi. Res fit in tenebris. Equidem cum filum ferreum exhibetur pelli, tum etiam videtur identidem emittere penicillum; ſed ſi attendas, eum penicillum non ad pilos dirigi cernes, ſed ad vitrum aſperum, dum ſubit filo ferreo inter fricandum.

Cæterum quæ hactenus ex tabula hac exiſtere videntur leges, cernet quivis *; conſtituo ego nihil, quamdiu ipſam omnigenorum corporum experimentis non amplificavero; cui rei maxime inhio 1. ut id ipſum certius intelligam, quæ corporum mutuæ affectiones in cauſa ſint ignis electrici ſui impertiendi, aut accipiendi alieni; 2. ut explorem, num natura corporum cohæſionem ordiatur aliquam, vel omnem ſimili vi, vel cauſa, qua ad tempus aliquod (quamquam ante plures annos vidi ego tenuem ex cera ſignatoria bracteolam tubo metallico ad plures menſes adhæſiſſe elec-

* Propoſita experimentorum tabula eorum ſummam exhibet, ſingularia ſingularis cujuſque experimenti adjuncti pertractationem poſtulant.

tricitatis

tricitatis vi) cohærere videmus corpora, quæ mutuo fricavimus; 3. ut pergam explorare, quanta parte illi etiam naturæ motus, quos chemicos dicimus, quando arte imitamur, igne electrico efficiantur, &c.

Theorema.

Chordæ semiangulorum, quibus divergunt duo fila, adeoque & vires, quibus ea fila divergunt, sequuntur directam simplicem proportionem densitatis ignis electrici redundantis in iis filis, aut raritatis ignis electrici deficientis.

Experimenti apparatus.

Fig. 2. I. In medio amplo experimentali theatro funiculis suspendo, & distraho, ne commoveri possit, tubum ex lamina ferrea FE longum pedes quatuor, latum pollices tres, pendentem libras tres.

II. Imo ipsius tubo puncto P, in extrema parte, necto pendulum PL. Imo alteri puncto, quod est in medio tubo, necto duo tenuissima argentea fila Bb, Aa emollita ad ignem, & probe tensa, ut pendeant recta, proxima, & parallela. Quo facile discedant hæc fila, in BA nectuntur alterum alteri perbrevi serico stamine; quo cerni eminus possint, singulis b & a necto levissima duo chartæ frustula, prospectanda contra pannum nigrum adverso muro adfixum.

III. In abaco eminus posito solidissimo, quo prorsus non trepidet, regula lignea lineam definio LM parallelam plano bB, Aa, in quo novi insistere fila, cum divergunt. Porro cum fila hæc sunt recte disposita, &

late disjuncta a corpore alio omni, divergunt in plano parallelo axi tubi FE.

IV. Tum regulæ adfixæ in LM adpono dioptras mobiles tres H I K. Eæ sunt acutissimæ, & rectæ tres acus infixæ cubis plumbeis.

V. Interea adsunt eminus duo homines A & B. Homo A separatus a solo, qui in tubum FE, attingendo auream bracteolam adfixam extremo ipsius G, (ne attrectatione tubi fila commoveantur) immittat electricitatem à catena, aut machina; alter homo B tenet extremum caput bacilli vitrei NO quo separatur tubus ferreus *fe* omnia similis, & æqualis tubo alteri FE instructus ipse etiam bracteola metallica *g*.

EXPERIMENTUM TERTIUM.

His ita comparatis, primo colloco dioptram H, ut radius visualis ex apice acus trajectus per filum verticale PL incurrat in duo stamina argentea B*b*, A*a*.

II. Tum homo A monitus eminus attingit bracteolam G; stamina divergunt ad angulum *b*′B A*a*′. Vix ipsa acquiescunt a prima vibratione, studeo ego dioptram K locare ita, ut radius visualis K*b*′ incurrens in chartam *b*′fili B*b*′ jam divergentis, trajiciat per filum verticale penduli PL.

III. Tum homo B monitus bracteolam *g* admovet bracteolæ G; electricitas tubi FE effundit se ad æqualitatem in tubum *fe*. Angulus divergentiæ filorum *b*′B, A*a*′ minuitur. Sollicitus dioptram I loco, ubi radius visualis ex ipsa incurrens in *b*″, trajiciat ipse etiam per filum verticale PL.

Experimentum autem tamdiu instauro, quamdiu dioptras recte constitutas iterum atque iterum observo, atque tum pergo ad calculum.

Calculus Theorematis demonstrationem suppeditans.

I. Metior diſtantias horizontales, HK, HI, HS, Sb. Tum HS inquam, ad HK, uti Sb ad quartum; atque quartus hic terminus eſt ſinus ſemianguli b'Bb, eſt enim intervallum horizontale duorum planorum verticalium, quorum alterum tranſit per puncta H, b & per filum verticale PSL.

II. Et ſimili analogia; uti HS ad HI, ita Sb'' ad quartum; quartus hic terminus eſt ſinus anguli b''Bb.

III. Ex ſinibus pronæ ſunt chordæ. Atque pluries repetitis eadem die, & diverſis diebus experimentis, ſemper chordam $b''b$ inveni quam proxime ſubduplam chordæ $b'b$, ut differentia ſit in perpaucis lineæ centeſimis, & differentiis æquatis experimentorum omnium, plane evaneſcat.

Theorematis demonstratio.

IV. Ex mechanicis, vis ſuſpendens grave in arcu circuli eſt uti chorda arcus. Itaque vires electricæ, quæ filum quodque divergens a filo ſocio ſuſpendunt in arcu circuli, ſunt ut chordæ angulorum, queis ipſum filum divergit a recta verticali. Quare quoniam ex experimento, chorda anguli b'Bb, exiſtentis ex igne electrico toto, eſt dupla chordæ anguli b''Bb exiſtentis ex igne electrico ſubduplo, efficitur has chordas, adeoque & vires, quibus fila divergunt, ſequi rationem ſimplicem, atque directam denſitatis ignis electrici redundantis.

Pro filis, in quibus ignis deficit, eadem eſt ex experimento demonſtratio. Alias demonſtrabo, ſimilem

lem existere etiam causam, scilicet experimenta ostendunt: quemadmodum ignis elcctricus redundans in filis expandit se circa fila, quin ex ipsis ineat in aërem habentem solum ignem nativum; ita ignem nativum aëris ambientis fila, in queis ignis nativus deficit, expandere se promptissime circa fila, quin ex aëre discedat.

Experimentum quartum.

Fig. 3. I. Abacum rectangularem ita loco, ut facie sua plana in plano sit horizontali S T.

II. Faciei ipsius adglutino laminam ex plumbo, quæ distet undique a margine pollices tres; unus solum ejus laminæ limbus extat ex margine abaci; appello limbum communicationis.

III. Quatuor lateribus tabulæ necto regulas ligneas quatuor lineam unam prominentes supra tabulæ faciem. Ita capacitas existit parallelopipeda, habens pro basi amplitudinem abaci, & undique lineam unam alta.

IV. In Aheno, liquo ad ignem, colofonium defæcatissimum, admiscens pondus æquale pulveris ex marmore cribrati ad summam subtilitatem, diu diligenterque calefacti, ne humor ullus reliquus insit, atque, ut omnis, qui potest, dispellatur aër.

V. Liquatam massam capacitati abaci infundo, atque ubi est necesse, cilindro æquare pergo; si alicubi fatiscat, consolidare satago ferro candente.

VI. Adglutino summæ, & mediæ hujus strati faciei laminam plumbeam distantem undique similiter pollices tres a margine abaci.

Atque ita paratum habeo abacum vere fulminantem, qui fulminantibus vitris præstat usus commoditate,

effectuumque magnitudine; etenim tempestate, etiam non ficcissima, quatit validissime, quod resinæ humorem respuant, quem attrahit vitrum. Præterea parari potest amplitudine quantalibet ad effectus quantoslibet.

Abacus meus fulminans indusium habet metallicum longum pollices triginta, latum pollices quatuordecim, & aërem clausum in breviore barometri crure BI scintilla sua tanta vi disjicit, ut totam mercurii columnam deprimat in eo crure ad sesquilineam, elevetque adeo ad altitudinem æqualem, in crure longiore, columnam mercurii totam * BK.

Experimentum III. usu venire potest in æstimandis ignis electrici pressionibus, seu viribus, uti appellant, mortuis; hoc, aut similia vires vivas possunt exponere.

Experimentum quintum.

Fig. 4. In globo vitreo *a b c*, qui rotatur in machina electrica, & fricatur pulvillo P, duplex communiter observatur lux ignis electrici: altera in *a*, ubi globus proxime discedit a pulvillo, altera in *c*, ubi globus proxime redit ad pulvillum; illam adeo appello lucem discessionis, hanc lucem reditus.

Hactenus cum Franklino opinabar, lucem discessionis existere ex igne electrico, qui a pulvillo trajiceret in vitrum, atque in ipso cumularetur, alteram ex eo igne cumulato, qui, cum globus proxime redit ad pulvillum, in hunc reflueret parte sua aliqua.

* Quoties experimentum hoc obvenit in facie mercurii, qui aëri subest vibrato, electrica lux micat. Nonne ea vibratione aliquis ab aëre electricus ignis in mercurium vibratur?

Secundam hujus opinionis partem video constare verissimam; si enim ignem frictione congestum in globum intercipiam quoquomodo, vitro, quod jam discessit a pulvillo, objiciendo ubilibet corpus deferens, pro eo igne sublato vel omni, vel aliquo, vel continenter, vel interrupte, omnis, vel parte aliqua, continenter, vel interrupte deficit lux reditus.

At in luce discessionis explicanda erravimus; non est enim ab igne electrico, qui a pulvillo ineat in vitrum, verum ipsa etiam æque, ac lux reditus, efficitur, parte ignis in vitrum frictione congesti in proximum pulvillum refluente.

Fig. 5. Etenim dum ceram signatoriam frico charta inaurata, existunt luces geminæ, B, A, tum quæ discessionis dicitur, tum quæ lux reditus appellatur, simillimæ ambæ inter se, simillimæ iis, quæ apparent in vitro. Fig. 6. Sed, cum frico charta nuda, geminæ luces iterum sunt A*a*, B*b*, similes inter se, sed iis prorsus dissimiles, quæ adparent in vitro, aut in cera signatoria perfricata a charta inaurata; illæ enim micant ad perbreve intervallum inter ceram signatoriam, & marginem chartæ inauratæ, atque juxta hunc marginem æmulantur seriem stellularum, istæ a margine chartæ nudæ, instar penicillorum, longe persequuntur faciem ceræ signatoriæ fugientem, aut redeuntem.

Nequit autem lux discessionis similis esse luci reditus in utroque experimento, & nequeunt geminæ in experimento uno esse dissimiles geminis in experimento altero, quin, quæ similes sunt in experimento uno, causa efficiantur simili, & quæ dissimiles sunt in experimentis duobus, causa efficiantur diversa. Sed lux reditus, cum fricatur globus vitreus, aut cum charta

inaurata

inaurata fricatur globus ex cera Hiſpanica, efficitur igne congeſto refluente in proximum marginem corporis fricantis; ergo & ſocia diſceſſionis lux ignis ejuſdem parte aliqua efficitur ex vitro, aut cera Hiſpanica in fricans corpus refluentis.

Iterum lux reditus, cum charta nuda fricatur globus ex cera ſignatoria, efficitur igne, qui a corpore fricante init in ceram orbatam igne ſuo; igitur & ſimilis diſceſſionis lux igne efficitur in ceram ſignatoriam refluente.

Forma earum lucum rem confirmat jam plane manifeſtam. Quæ nempe luces geminæ in margine chartæ ſunt, ceu ſeries ſtellularum, eas decet igne effici in proximum marginem ineunte, quæ ex eodem margine erumpunt ſimillimæ penicillis, eas decet effici igne prodeunte.

P. S. Addo experimentum elegans, uti videtur, commodum, neque infacundum. Zonam ex panno ſerico villoſo nigroque advolvo cilindro vitreo, qui fricatur in machina electrica, diſtendo, adſuo, pilis frico, qui extant ex pelle leporis; frictione non ita valida, electricitas exiſtit in catena vehementior, quam quæ exiſtit ex frictione vitri, ſed huic contraria. Frictio modica commoditatem facit in experiendo, vehementior electricitatis, & experiendi commoditatem auget, ut & experimentorum effecta. Electricitatis in vehementia contrarietas quæſtionem finit. Uti enim video, qui electricitatem reſinæ negabant contrariam electricitati vitri, abutebantur ejus electricitatis debilitate.

XVI. *Pr-*

XVI. *Proposal of a Method for measuring Degrees of Longitude upon Parallels of the Æquator, by* J. Michell, *B. D. F. R. S.*

Read May 8, 1766.

THERE have been already several attempts made towards discovering the figure of the earth, by measuring the length of a degree of the meridian in different latitudes: now if these measures had been sufficiently accurate and numerous, and we could also depend upon the uniformity of the earth's surface, we might then immediately discover from them the form sought; but these measures, not agreing exactly to any certain rule, leave us still in some degree at a loss. It is therefore much to be wished, that more measurements of degrees upon the meridian were to be made, in order to determine with greater accuracy a question of this importance.

But what would tend yet more to determine this matter, would be the measurement of degrees of longitude, as well as those of latitude. Astronomers have indeed expressed their wishes that this might be done; and though no attempt has been hitherto made towards it, yet as it is probable, that such measurements may some time or other take place, it will not be amiss to suggest a method, which will admit of more exactness than any I have seen proposed for this purpose, all of which, depending upon an observation of the time, are therefore liable to an error of fifteen seconds of a degree for every second of time; but the method, I mean to recommend, stands upon the same foundation with the

 measure-

meaſurement of a degree of the meridian, and, the inſtruments being equally good, and the number of miles to be meaſured the ſame, the exactneſs of it, to that of a degree of the meridian, will be in the proportion of the ſine of the latitude to the radius very nearly.

In TAB. V. Fig. 7. let AB repreſent the æquator; P the pole; DLEF a parallel of the æquator; PEC a meridian paſſing through the ſtation E; PLMN a meridian paſſing through another ſtation M; and let AMEB be a great circle cutting the meridian PEC at right angles in the point E.

Then in the ſpherical triangle AMN, right angled at N, we ſhall have R : Coſ. AM [Sin. ME] : : Tan. MAN : Co-Tan. AMN: hence $\frac{\text{Tan. MAN}}{\text{R}} \times$ Sin. ME=Co-Tan. AMN; but Tan. MAN, being the Tangent of the latitude of the given place E, and therefore given, the quantity $\frac{\text{Tan. MAN}}{\text{R}}$ will likewiſe be given, and greater or leſs than unity in the proportion of the Tan. of the latitude to the R. The Co-Tan. therefore of the angle AMN, that is the Tan. of the complement of the angle AMN to 90°. will be greater or leſs than the Sine of the Arc ME, in the proportion of the Tan. of the latitude of the place, to the R. And conſequently, whilſt the Arc ME is ſmall (in which caſe the Sine, Arc, and Tangent differ very little from each other) the angular deviation of the interſection of the meridian PLMN with the great circle AMEB, from a right angle, will contain more or fewer degrees, &c. than the Arc ME nearly in the (ſame) proportion of the tan. of the latitude of the place to the R.

By

By this means then, the latitude of the place and the angle PME (contained between the meridian PMN and the great circle AMB) being given, the length of the Arc ME will likewiſe be given, with great exactneſs. But I muſt obſerve, that as the angles PEM and PME muſt be taken by the obſervation of ſome ſtar near the pole, they will be leſs accurate, when reduced to the plain of the horizon, than at the pole, in the proportion of the Sine of the diſtance between the pole and zenith, that is the Coſ. of the latitude, to the R, which with the proportion juſt mentioned of the Tan. of the latitude to the R, makes the accuracy of this method upon the whole, when compared with that of the meaſurement of a degree of the meridian, in the proportion of the Tan. multiplied into the Coſ. of the latitude, to the ſquare of the R. very nearly; but the Tan. of any angle into its Coſ. is equal to the Sin. into the R. whence this proportion is the ſame as the Sin. into the R. to the ſquare of the R. and dividing both by the R. ſimply as the Sin. of the latitude, to the R. as above.

Having got the length of the arc ME, of a great circle, in degrees, &c. together with the diſtance of the two ſtations M and E, it is eaſy to conclude from thence the length of a degree of the parallel of latitude, at the place of obſervation, which will be the ſame, without ſenſible error, as it would be, ſuppoſing the earth was an exact ſphere, to the ſame ſcale, with the degree of a great circle juſt found.

For in Fig. 8. let APB repreſent a ſection of the earth through its axis PCH; ACB an æquatorial diameter; AD the radius of curvature at the point A; and PH the radius of curvature at the

point P; DFH the evolute of the curve AEP; EF the radius of curvature at the point E; (which we will ſuppoſe to have the ſame latitude with the point E in Fig. 7.) and let EF be produced till it cuts the axis PH in G: then with the radius EG and centre G, deſcribe the arc IEK, which will be the leaſt circle, that can touch the curve AEP at the point E, without cutting it. Let now the curve PEA, the line EG, and the arc IEK revolve about PH as an axis, and, PE being equivalent to PE in the former figure, the point E in the latter figure will deſcribe the parallel DEF in the former; AEP at the ſame time deſcribing the ſurface of the earth, and IK deſcribing a portion of a ſphere, which will be every where a tangent to the parallel DEF, and whoſe centre will be G. The curvature therefore of this ſphere will be leſs than the curvature of the earth, in the direction of the meridian, at the point E, as the radius GE is greater than the radius FE; but this, in moderate diſtances, can cauſe no ſenſible error. The difference between AD the radius of curvature, at the point A, on the earth's ſurface, and the line AC, according to that hypotheſis, which makes it the greateſt, does not exceed one ſixtieth part of the whole, and upon the ſame hypotheſis, the part FG of the line EG, ſuppoſing E to be in the latitude of 45°, would not exceed $\frac{1}{130}$ part of the whole. If then we take any other point upon the ſurface of the earth as M, at a ſmall diſtance from E, the diſtance between that point and the ſphere deſcribed by the arc IK, will be only $\frac{1}{130}$ part of the verſed ſine of the arc EM, and the perpendicular ſtanding upon the ſurface of the earth at M, will be inclined to the perpendicular

ſtanding

ſtanding upon the ſphere in an angle, which is equal to $\frac{1}{130}$ part of the angle ſubtended by the arc EM. And in higher latitudes theſe quantities will be ſtill leſs. Let us now return to Fig. VII. and ſuppoſing the point E to be ſituated in latitude 45°, let the arc EM cutting PE at right angles, conſiſt of 2° (near 140 ſtatute miles) then will the ſide PM, of the triangle PME, conſiſt of 45° 2′ 5″$\frac{1}{2}$, and conſequently, if LM in Fig. 7. be ſuppoſed to correſpond to EM in Fig. 8. the diſtance of theſe two points E and M, in the latter Fig. will be only 2′ 5″$\frac{1}{2}$, the $\frac{1}{130}$ part of the verſed ſine of which is a little more than $\frac{1}{3}$ of an inch, to the radius of the earth, which will therefore be the diſtance of the point M upon the earth's ſurface, and the point of the imaginary ſphere, deſcribed by IK, immediately over it. Hence alſo, the inclination of the real perpendicular at M, and the imaginary one ſtanding upon the arc IK, at the ſame place, to each other, will be ſomething leſs than a ſecond, a quantity in itſelf almoſt too ſmall to be regarded, unleſs the inſtruments made uſe of are both very large and very excellent in their kinds, and which, being wholly in the plane of the meridian, will produce an error, that muſt be perfectly inſenſible, with any inſtruments whatſoever, in an obſervation of the angle PME, Fig. 7. which will therefore, to all intents and purpoſes, be the ſame, as if the curvature of the earth in the direction of the meridian, and in the direction of ME or LE were accurately the ſame.

I have ſuppoſed the arc ME, to ſtand at right angles to the meridian PE, which paſſes through one of the extream ſtations; the method here propoſed is,

however, liable to the leaſt error, when the meridian cuts the arc to be meaſured at right angles in the middle of it; but this makes ſo very ſmall a difference, that it is not worth regarding; nor is it indeed neceſſary, that the arc ſhould not deviate two or three degrees from right angles with the meridian, at that end where it cuts it moſt nearly at right angles, in caſe the ſituation and circumſtances of the country ſhould make this more convenient, the errors, that would be occaſioned by ſuch a deviation, being too ſmall to affect the concluſion. And if this deviation was ſtill much greater, and the length of a degree of the meridian at the ſame place was known, it would be very eaſy to make the neceſſary corrections.

It will perhaps be objected, that the method above propoſed depends, in ſome meaſure, upon time, as well as others, the finding of the meridian not being to be performed without it; but I muſt obſerve, that the motion of the pole ſtar, by which I propoſe to find the meridian, being ſlower than that of a ſtar at the æquator, nearly in the proportion of 30 to 1, this method will admit of an exactneſs greater in the ſame proportion (except the reduction of the Sin. to the R. before mentioned) than thoſe obſervations, by which we endeavour to find the difference of the longitude of two places, by the difference of the time of the ſun, or a ſtar's coming to their reſpective meridians.

The method above propoſed will likewiſe require different inſtruments from thoſe commonly in uſe; but admitting, that inſtruments of equal radius are capable of equal exactneſs, this method will admit of the ſame exactneſs with the obſervations of a degree of the meridian, except the before-mentioned limitation.

tion. Nor would the inſtruments for this purpoſe, if well contrived, be either leſs portable, or more expenſive, than thoſe for meaſuring a degree on the meridian; the ſame teleſcope which would be neceſſary for finding the meridian, would ſerve likewiſe for tracing the arc of a great circle; but it is beſide my preſent purpoſe to conſider any farther what kind of inſtruments would be moſt proper upon this occaſion, or what would be the beſt manner of conſtructing them.

I muſt not, however, diſmiſs the preſent ſubject, without obſerving, that, by means of the above-mentioned method, a country not too near the æquator, nor attended with any other unfavourable circumſtances, might be laid down with wonderful exactneſs. By running a great circle nearly Eaſt and Weſt through the midſt of it, we ſhould get the longitude of all the places, the great circle paſſed over; and if, by means of the meridian teleſcope, we ſhould trace meridians through a few of theſe places, as far North and South, as the ſurvey was intended to be carried, we ſhould then have a number of ſtations, in ſeveral parts of the country, whoſe longitudes, with reſpect to one another, would be very accurately determined, and to which other places might eaſily be referred, when the length of a degree of longitude in thoſe ſituations was known.

XVII. *Obſervationes de* Aſcaridibus *&* Cucurbitinis, *& potiſſimum de* Taenia, *tam humana quam leporina.*

Read May 15, 1766.

SI omnes pervolvantur auctores, qui de vermibus corporis humani tractaverunt; vix de plerifque, fcilicet de taenia, afcaridibus & cucurbitinis, aliquid certe inde intelligitur. Non enim concipitur an taenia ex afcaridibus, juxta Couletum, vel ex cucurbitinis, juxta plerofque, agglutinatis & concatenatis conftet; num potius fit vermis fuae & fingularis fpeciei, ifque vel folitarius, vel multiplex; tuncque an cucurbitini non fint totidem taeniae annuli fingulares, ab ea feparati, vel alia vermiculorum fpecies; praeter quas, aliae reftant difficultates non leviores, unde, exempli gratia, originem ducant, imo quinam fit fingulorum character naturalis, aut figura externa, quod poftremum dubium vel a defcriptionibus, figurifve erroneis, vel a varietate naturali ipforum vermium pendet.

Quae, ut dilucidentur, miffis fabulis, & opinionum commentis, neceffe eft horum animalculorum defcriptionem unice a natura depromere, & fuper iis obfervata multiplicare; unde tandem eorum doctrina nitebit.

Cūm mihi, quod non multis contigit, omnes vermium corporis humani fpecies, vel faltem taeniarum fruftula videre licuerit, & praefertim cum e leporis inteftinis taeniarum binarum extremitates obtinuerim, inde occafione fruor fingulari nonnulla de hifce vermium fpeciebus exhibendi, quæ ad taeniae hiftoriam naturalem

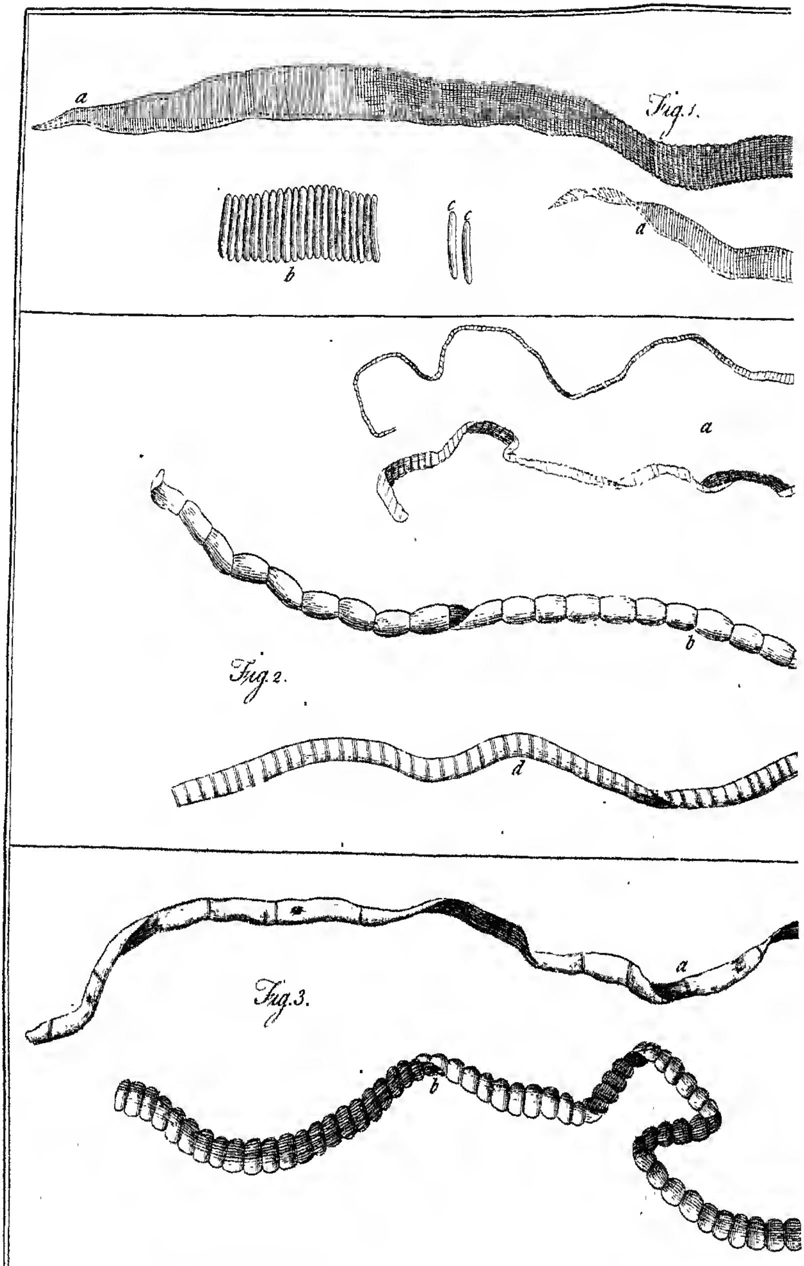
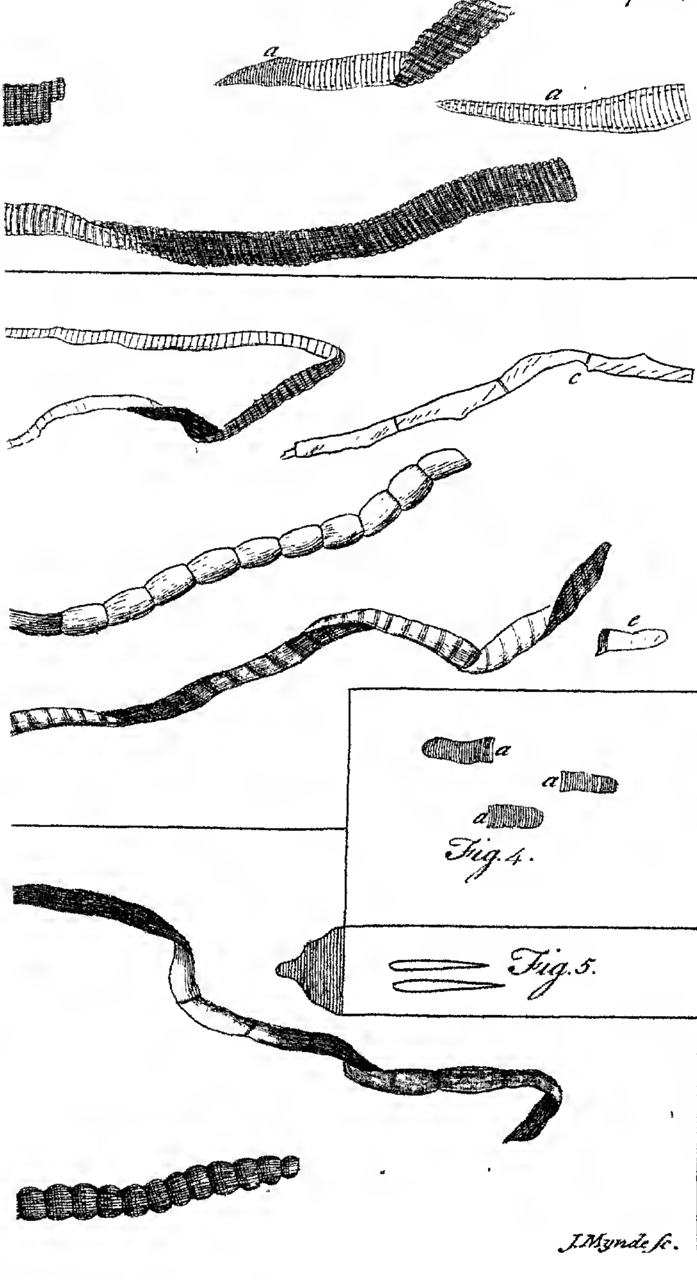


Fig. 1.
a
b
c c
a
a
b
Fig. 2.
d
Fig. 3.
a
b



J. Mynde sc.

Fig. 2. exhibet fruſtula taeniae, quae, aſſumpto, veſperi, mercurii dulcis cum ſaccharo triti ſcrupulo uno, expulit poſtridie pulvis purgans, mulieri noſtrati exhibitus. Superior portio *a*, extremum lineare exhibet. Mediae portionis *b* ſegmenta triplo, alterius *c* circiter quadruplo longiora ſunt quam inferiores *d*, ita ut haec videatur uni extremitatum propinquior.

In annulo, ſeu ſegmento ſeparato *e*, ſpecies ſulci ſeu hiatus longitudinalis ab uno extremo eſt, qua parte alteri jungebatur.

Fig. 3. duo alia repraeſentat, alterius taeniae, a praecedenti non multum diverſae, fruſtula; quorum ſuperioris *a* ſegmenta ſunt tenuiora, laeviora, fere quadruplo longiora quam inferioris *b*, cujus annuli magis longi quam lati, aſperiores, ſpeciem verrucarum, aut mammillarum, alias in lateribus, alias in parte plana, exhibent.

Vaſculum, ſpecie fili caerulei, aut purpurei, quod in medio longitudinis ab uno ad aliud extremum per taeniam decurrere dicitur, non datur in hiſce taeniis; ſed apparentia velut vaſculoſa obſcurior in quolibet annulo conſpicitur.

Fig. 4. exhibet vermes cucurbitinos, ita ab Hippocrate, ob quamdam, ipſi viſam, ſed minime realem, cum ſeminis cucumerini aut cucurbitini ſimilitudinem; horum extremum latus *a*, ſeu baſis, ſpeciem ſulci ſeu hiatus habet.

Haec ſpecies magnam affinitatem habet cum ſegmentis taeniae ſeparatis, ut videre eſt comparando in Fig. 2. litt. *e*.

Tales habeo quamplurimos, ut a juveni jamdudum excernuntur.

Fig.

Fig. 5. aſcarides repraeſentat, quales vidi innumeros. Hic vermis, circiter dimidium pollicem longus, corpore donatus eſt terete & laevi, opaco, candido, in duo extrema deſinente, quorum aliud tenuius quam medium corpus, obtuſum, ſubſtantiae candidae, opacae; aliud ſenſim magis acuminatum & tandem capillare, pellucidum, excolor.

His obſervatis, nonnullae animadverſiones ſequentur.

Primo nil aſcaridibus cum taenia aut cucurbitinis commune videtur; ſed ſingularis vermium ſpecies cenſendi ſunt.

Qui vero aſcarides Couleto audiunt, ii ſunt, aliorum cucurbitini; quorum excretio, ut taeniae ſignum, ab omnibus habetur. Inde vel ex hiſce coalitis formari taeniam, quae communior ſententia eſt, vel eos non niſi taeniae ſegmenta ſeparata eſſe, apparet.

Prior opinio, maxime ſpecioſa, ipſo figurarum intuitu evaneſcit. 1. Enim ipſe ordo & diverſitas annulorum, & praeſertim exilitas annulorum extremitatis, *a* (Fig. 2.) formationem ex cucurbitinis concatenatis renuit; hos itaque potius non veros vermes, ſed taeniae annulos ſeparatos arguit. 2. Comparatio taeniae leporinae hoc valde confirmat. In ea omnes annuli ſunt longitudinales; atque cum ſulcus *a* cucurbitinorum pro eorum ore accipiatur, repugnat ſegmenta Taeniae leporinae fuiſſe vermes, cucurbitinis noſtris affines, quorum os tota longitudine decurreret. 3. Puncta illa, in ſegmentis extremitatum taeniae leporinae, eorum mechaniſmum teſtantur diverſum quam ſegmentorum intermediorum, in quibus talia puncta non cernuntur, ita ut non eadem ſit eorum fabrica originalis, nec per

 conſequens

consequens communis origo a consimili verme. 4. Divisio annularis in aliis vermium speciebus visa est; nec a tali concatenatione hos ortos suspicatum est, nec per consequens taeniam oriri suspiciendum; actuum Edinburg. Vol. II. prostat exemplum vermis rotundi pariter annularis. Linnaei (in Faunâ Suec.) taenia articulata teres, aliud est similis constructionis exemplum. Haec, aliaque, non ab insueta naturae lege, qualis plurium animalium redactio in unum esset, sed naturalem esse constructionem testantur.

De taeniae capite bipartiuntur auctores. Jam celebris Tulpius (1) reprobravit taeniae descriptoribus, quod eam semper mutilam & tantum capite tenus descripserint. Caput alii etiam agnoverunt, ut Andry (2), Raulin (3), Thomas (4); caput Andryanum, speciem porri aut verrucae; Tulpianum velut caput colubrinum; Raulinum specie rostri, cum duobus punctis nigris ad duo latera; Thomas, ut punctum nigrum, quale in quorumdam fructuum insectis apparet.

Quoad Tulpium, notandum quod non propria autopsia, sed aliorum medicorum testimonio, & sculptoris delineatione id admittat.

Tot diversitates in capite pingendo & describendo, an non ejus realitatem minus certam faciunt? an non praeoccupatio potuit dilaceratae forsan extremitatis appendices, specie rostri aut capitis, figurare? maxime quum Raulin & Thomas, nonnisi microscopio adjuti,

(1) Obs. Med. lib. ii. cap. 42.
(2) Gen. des vers.
(3) Maladies par les variations de l'air.
(4) Journal de Medecine, Juillet, 1765.

caput

caput depinxerint, quum Tulpii, & Andry, summe diversa capita videantur oculis nudis conspecta fuisse.

Alii observatores, inter quos ego, nil capiti simile vidimus; truncatum forte potuit esse ab extremo, quale in Fig. 2. Fateor tamen leporis taeniarum extremitatibus quatuor deficiens caput mihi persuadere has taenias dicto organo destitutas fuisse visibili, quum videatur iis extremis nihil truncati fuisse. Revera hae taeniae dilaceratae erant in partibus intermediis, quod domestici incautela & initio corruptionis viscerum contingere potuit, quum is lepus jam a decem diebus asservatus fuerit. Sed extremitates visae sunt sanae & integrae, nec capitis, vel etiam separati, vestigium reperire potuimus. Accedit quod Linnaeus, de taenia intestinorum agens (1), referat se taeniam invenisse in ochra acidulari Jaernensi; unde concludit, ex ovis vermium, una cum aqua haustis, tales & alios vermes in corpus pervenire; nec de capite mentionem facit hic egregius historiae naturalis scrutator. Haec rem indecisam faciunt, novaque requirunt exactiora observata.

Alia quaestio est num solitaria sit taenia, vel multiplex? Quatuor extremitates (Fig. 1.) arguunt duas saltem fuisse in lepore. Duae pariter repertae sunt in fele, altera in stomacho, altera in duodeno (2); Tulpius (l. c.) ter integram a muliere quadam excretam asserit.

Alia discussio est num multiplices sint taeniarum species? alia annulis longis, ut depinxit Coulet; alia brevioribus, ut Tulpius; alia articulis

(1) Syst. Nat. Obs. in R. Anim.

(2) Mem. de l'Acad. R. des Sciences de Paris, Vol. 17. pag. 484. Edit. d'Amsterdam.

verſus extrema exilibus, in medio longioribus, quales taenia Hippocratis & Andry; quales videntur fuiſſe taeniae hic depictae, tam humanae quam leporinae. An forte duae priores ſpecies ſtabilitae ſunt, ſecundum fruſtula, quae prodiderunt, extrema aliis, aliis media? quod probabile videtur. An non etiam aliae aliis latiores, quod a diverſitate pabuli nutritii pendere poſſet? & hoc veriſimile videtur. Interim leporinae & humanae taeniae diverſitas ſpecies diverſas oſtendit.

Unde tandem originem ducunt taeniae? Certe, ut jam ex Linnaeo deduxi, tam taeniae, quam alii vermes inteſtinales, videntur ex ſimilium vermium ovis, cum aqua aut edulibus deglutitis in inteſtina pervenire, ubi proprium alimentum nacti creſcunt & vigent, probabiliterque ſpeciem propagant. Quae ovulorum taeniae deglutitio haud difficilis videtur, conſiderando taeniam ex praedictis auctoribus reperiundam eſſe in ovibus, canibus, tinca piſce, fele, lepore, atque in ipſa ochra; ex quibus facilis communicatio, ſeu per aquam, ſeu per alimenta, quibus depoſita ſint ova, deducitur.

Ut veri limites non excedam, his ſuperſtare congruum duxi, donec plura horum vermium, praeſertim integrorum, obſervata omnibus difficultatibus ſatis faciant.

Haec interim, quantumvis mediocria, ſperans aliquantum lucis huic materiae afferre poſſe, perilluſtri Regiæ Scientiarum Societati offert ſubmittitque,

Joannes Philippus de Limbourg, M. D.
Et Regiae Scientiarum Societatis
Monſpelienſis Correſpondens.

XVIII. An

Fig. 1.

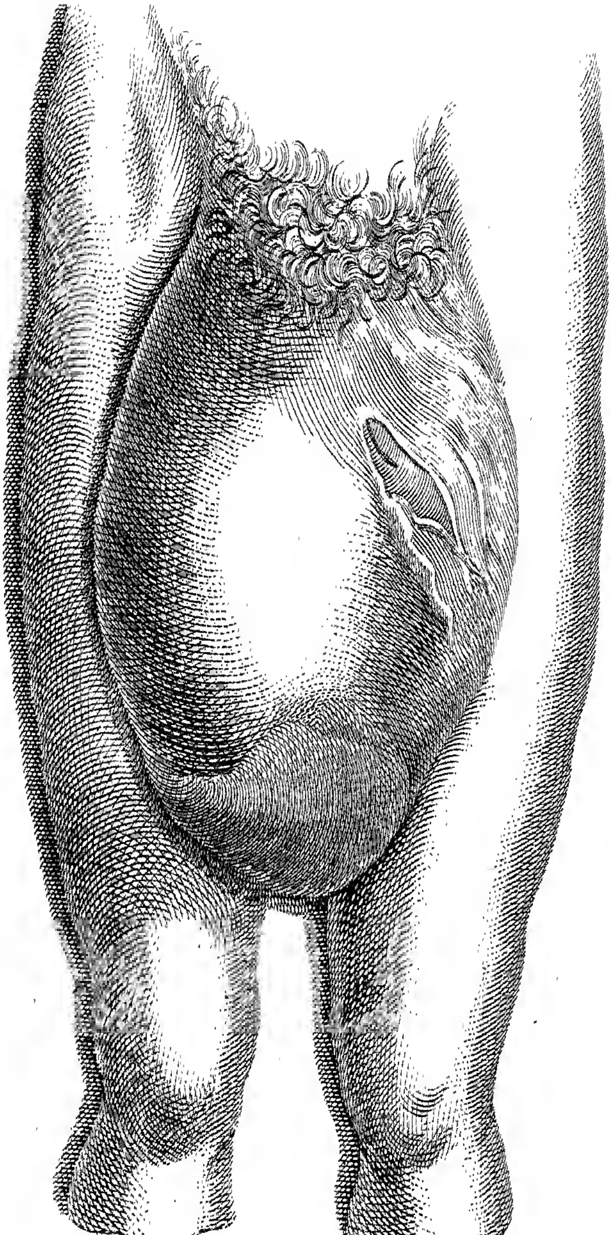
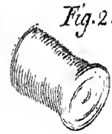


Fig. 2.



Received April 24, 1766.

XVIII. *An Account of an uncommon large Hernia, in a Letter from Dr.* George Carlisle, *to the Right Rev. the Lord Bishop of* Carlisle, *F.R.S.*

Carlisle, April 18, 1766.

My Lord,

Read May 14, 1766.

WHEN I shewed you the drawing of an uncommon large hernia at Rose, you were pleased to say, you should be glad to have the history of it, and of what occurred in examining the body after death, in order to communicate it to the Royal Society: from that time I determined to draw out the case, but have been prevented by various other engagements, till now, that I take the liberty to present it to your Lordship; and shall be extreamly rejoiced if it prove agreeable to you, and the learned body; with it I inclose an outline of the drawing, Tab. VII. Fig. 1. and an explanation, which may make the description more intelligible. I was sorry, that for want of a proper draftsman, my good friend the captain being out of town, I could not have the situation of the stomach, with the other parts left in the abdomen, taken; but my painter was so squeamish, it was with difficulty we got the outward appearance taken from the dead body.

We

We have had the drieſt ſeaſon here I have known; thro' the whole of laſt year, only 19.705 inches of rain fell; and in the three laſt months of this, only 1.689 inches, including what melted from the great fall of ſnow on the 11th and 12th of February, into my receiver: the ſnow lay then 9 inches perpendicular, upon a level bed in my garden. I am, with great reſpect,

My Lord,

Your Lordſhip's much obliged,

and moſt obedient Servant,

George Carliſle.

JOHN HALLOWDAY, an out-penſioner of Chelſea, aged near 80, having entered very young into the army, and undergone ſeveral hardſhips in the campaigns under the Duke of Marlborough, upon his return to England from Flanders, at the concluſion of the war, firſt perceived a ſmall tumor in the right ſide of the ſcrotum and groin. This he carefully concealed, to avoid the ſcoffs of his companions, and leaſt it might be the occaſion of his diſcharge, which he dreaded, and wanted to avoid; as he found no other inconvenience from it, but what its bulk occaſioned, nor ever had pain, vomiting, obſtructions to ſtools, or any other ſymptoms of a ſtrangulated hernia. From that time, however, it continued to increaſe in bulk; and from that, and its weight, grew daily more inconvenient to him, inſomuch, that about the year 1725, being

unable to go through the duty of a ſoldier, he was admitted to the out-penſion of Chelſea hoſpital. Its ſize was then ſuch, that he was obliged to have a particular bag made in the forepart of his breeches, to enable him to carry about its weight, and always wore a leathern apron to conceal its figure. For ſix or ſeven years before his death, the weight and bulk of the hernia had made ſuch an alteration, in the outward appearance of the parts about the ſcrotum, that the penis was entirely buried in the tumor; a ſmall oval opening only was left, out of which the urine was diſcharged: this opening was ſometimes excoriated, from the acrimony of the urine, as the penis could not be extracted to throw it off, nor the glans be made to appear by any endeavours: after death, it could be protruded no farther outwards, than as it is ſhewn in Fig. 2. A year or two before his death, after a cold, and fretting the part by too much walking, the urine had brought on a conſiderable inflammation, which mortified to a large extent, one conſiderable eſchar, formed upon the anterior and moſt depending part of the bag, one leſs on the right ſide where it touched the thigh, and a third behind; yet all caſt off and healed kindly, by the help of the bark, warm dreſſings, &c. Except from this accident, in the latter years of his life, he was not ſubject to any other complaints than are common at his years; ſuch as dimneſs of ſight, catarrhous coughs, ſhortneſs of breath upon motion, ſwellings of his legs occaſionally: and he wore off at laſt by a gentle decay, having all along had as good an appetite, and digeſtion, as could be expected at his time of life; regular diſcharges, both by ſtool and urine; very rarely vomitings, except from over-

loading

loading his ſtomach; purgatives, and every other medicine, operated as regularly upon him, as upon any other perſon. He was a well-made man, rather above the middle ſize; was as corpulent, and had as much ſtrength, as moſt of his years, until within a very little time of his death.

His caſe having nothing particular in it, but as far as the contents of the abdomen and ſcrotum were concerned; it was not thought neceſſary to carry the examination of his body farther than through them.

The large hernial bag I had meaſured, as exactly as I could, about a year before his death; and found its length, from the os pubis to the moſt depending point, 15 inches; its greateſt breadth, while it lay ſupported by the thighs, 17½ inches; and its greateſt circumference 34 inches: but in the body, the day after death, its length, from the pubis to the moſt depending part, was only 13 inches; its breadth, to the part where it fell in between the thighs, 12 inches; its circumference round the thick, or ſmalleſt part, where it deſcended from the pubis, 19 inches; and round its large circumference, 27 inches. It was covered with the common integuments of the ſcrotum; but at its lower and poſterior parts, the cellular membrane, or dartos, was reduced to an almoſt cartilaginous hardneſs, where the weight, both in ſitting, ſtanding, and lying, had the greateſt effect: the cicatrices alſo, where gangrenous ſloughs had been caſt off, were of an equal firmneſs, and hardneſs under the knife. The teſticle of the left ſide was plainly to be felt, at the prominent part, above, and to one ſide of the opening for the penis, not far from its natural ſituation: the right teſticle was obſcurely to be felt, a little above the

loweſt

loweſt and anterior point of the bag. Beſides what appeared upon the front view of the bag, a large portion of it, like a ridge, extended backwards, where the ſpace betwixt the thighs allowed it more room; they being rather more concave than uſual, inwardly, towards each other; and more diſtant, from the conſtant preſſure they ſuſtained. The colour of the bag was the ſame as that of the other parts, except where the mortified ſloughs had been caſt off; where it was of a ſhining white. Upon opening the abdomen, the liver appeared rather large, and farther extended over the left ſide than uſual. The gall-bladder was ſmall, with a little diluted bile in it. None of the inteſtines appeared, but a portion of the colon, towards the anterior edge of the pelvis, on the left ſide; where it made two inflections, much in the way as the loweſt turns of the inteſtines are ſhewn to do, below the omentum, in Euſtach. TAB. IX. from theſe it went downwards, and backwards, into the pelvis, to make its laſt curve, and be continued into the rectum: which, with that laſt curve of the colon, was in its natural place and direction. The ſtomach, inſtead of an horizontal, had a longitudinal poſition; its large, and here upper extremity, being placed behind the left lobe of the liver, cloſe to the diaphragm, and its large convex ſide lay along the left ſide of the abdomen; it deſcended to nigh the creſt of the os ilium, from whence it turned over the inflection of the colon, before-mentioned, acroſs the pelvis, to the large hernial aperture, in the right ſide; within the verge of which, it ran downwards about an inch, then aſcended, and made a ſemicircular turn to the pylorus, which mounted towards the abdomen; from

 thence

thence the beginning of the duodenum made another turn, to defcend into the hernial bag; immediately below which, viz. juft within the opening of the hernial bag, the ductus communis choledochus entered it; and feemed the caufe which kept it from falling further into the fac. From this, the remainder of the duodenum, and all the other inteftines, were entirely contained in the hernial bag, to nigh the extremity of the colon, before-mentioned. The duodenum, after entering the fac, firft ran a little downwards, and backwards, then horizontally, and laftly upwards, to within the edge of the fac, towards the abdomen; from thence the tegumen proceeded backwards and downwards, and then formed, with the ileum, pretty nigh their ufual convolutions, about the middle of the tumor, as they fhould have done in the abdomen. The caecum had a very fmall appendix, but was itfelf very large; as was the colon through its whole length, while contained in the fac; that part of it, which returned into the pelvis again, being much fmaller, even only of the dimenfions of the fmaller inteftines: the length of the colon too feemed more than ufual. The caecum began in the lower part of the bag, and from thence the colon kept pretty nigh the courfe it fhould have kept, if the bag had been the abdomen, for a great part of its length; running up, from the caecum, along the right fide of the bag, to nigh the pubis, and then croffing over towards the left fide, before the duodenum, to the left edge of the hernial aperture; at which place, flipping behind the lower extremity of the ftomach, it appeared in the pelvis, croffing over to its left fide; from thence to follow the courfe before defcribed. The pancreas lay in a longitu-

longitudinal direction, along the concave arch of the ſtomach, through its whole courſe; and was placed before the bodies of the vertebrae. The ductus choledochus, beſides its great length from the liver, to within the hernial bag, was of ſuch a width, as eaſily to admit a middle-ſized finger, being about 2½ inches in circumference: in ſome parts of its courſe, it was little inferior in width to the gall bladder, in this ſame ſubject. The kidneys were rather ſmall, in general ſound, except that ſome few hydatides were here and there fixed upon their outward ſurface, and that two or three ſteatomatous tumors, of about the ſize of a pea, and white, were in the ſubſtance of each; but not riſing above their ſurface: they were each in their proper ſituation; the left lay behind the ſtomach, and was leſs, probably from the preſſure it was expoſed to. The ureters and bladder were in their uſual ſituation; the bladder was no way engaged in the hernia, and a catheter was pretty eaſily introduced, through the concealed penis, into it. The ſpleen was ſmall in its natural ſituation, and ſound. The meſenteric glands were numerous, large, hardened, and ſurrounded with a fat of a deep yellow, as was the pancreas; no omentum appeared; its place ſeemed ſupplied by the fat, interſperſed among the glands, and pancreas. The teſtes were of a natural ſize, but looſe and flabby, and had many varicoſe veins upon their ſurface: the right, which was ſo much out of its proper place, was the leaſt, and laxer of the two: the ſpermatic veſſels belonging to it were large, through the great length they ran. The ſac and inteſtines were adherent, almoſt at every point of their contact; in ſome places ſo firmly, that they were with great

 difficulty

difficulty ſeparated, and often not without danger of tearing: the inteſtines alſo adhered in the ſame manner, to one another; all, by means of a firm cellular membrane. The containing bag was very firm, thick and ſtrong, as obſerved before. Its aperture, at the right ring from the abdomen, was ſo wide, as readily to allow a middle-ſized hand to paſs through it, from the abdomen, for a ſmall ſpace, betwixt its anterior edge, and the convolutions of the lower extremity of the ſtomach, and the ſemi-circular turn it made to the pylorus, with the beginning of the duodenum from thence, and the other extremity of the duodenum, before the jejunum commenced, and that part of the colon which returned into the pelvis; all of which were lodged in the very aperture: ſo that the ſpace left unoccupied by theſe parts could not be much leſs than 8 inches in circumference: notwithſtanding which, very little of a watery fluid was found in the ſac: indeed it would not have had a very eaſy admittance, from the many adheſions formed betwixt the ſac and its contained parts, a little below the opening from the abdomen.

The above is a tedious perhaps, but a circumſtantial, and juſt repreſentation of this extraordinary caſe: which I ſhall not, at preſent, lengthen by deductions or reaſonings; farther, than to admire the exquiſite compoſition of that moſt admirably formed machine, which could bear ſo great an alteration in its parts, without a manifeſt impediment to its moſt material actions: ſeeing here, life, and even health, went happily on, through a great length of years, though the whole ſyſtem almoſt of the inteſtines had been, for many of theſe years, without the reach of the

action

Fig. 1.

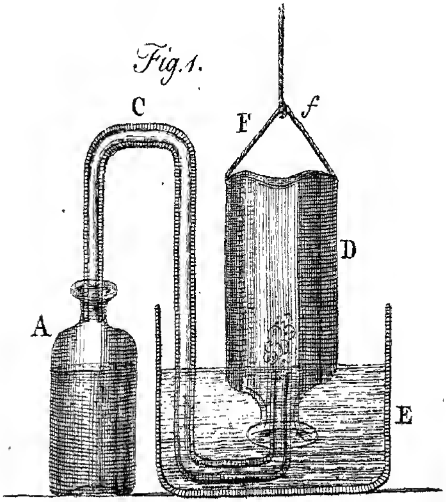


Fig. 2.

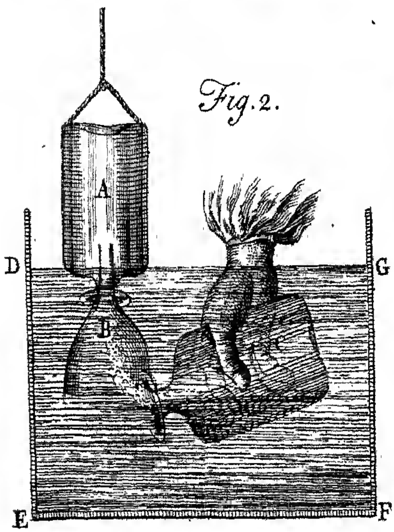
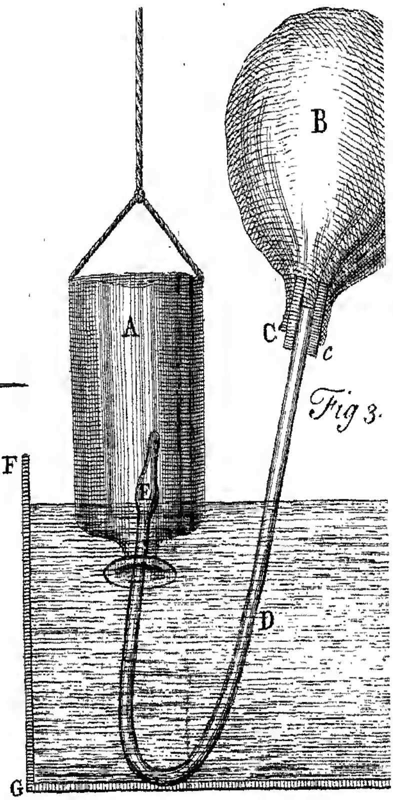


Fig 3.



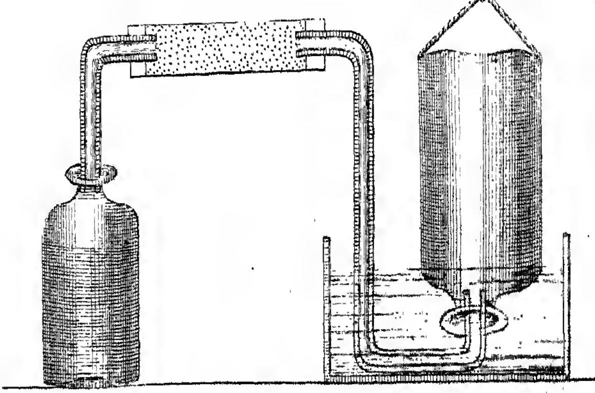


Fig. 5.

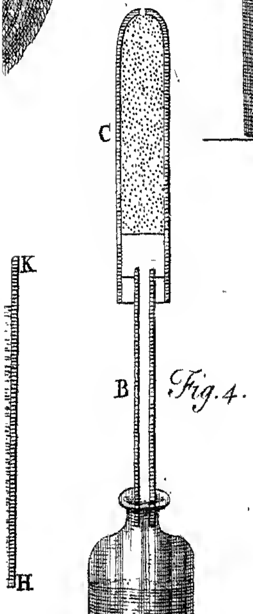


Fig. 4.

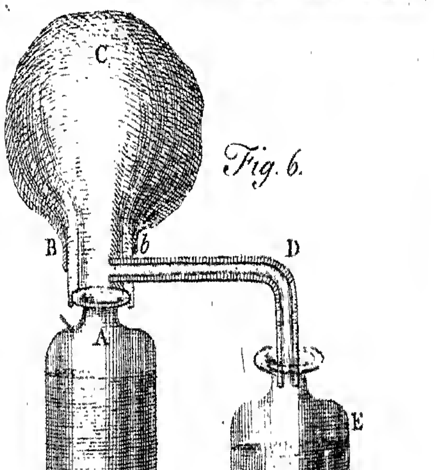


Fig. 6.

action of the diaphragm and abdominal mufcles, and of the fotus of their generally neighbouring parts: requifites, as has been imagined, towards the carrying on their feveral functions, for the benefit of the animal oeconomy.

George Carlifle.

Received May 12, 1766.

XIX. *Three Papers, containing Experiments on factitious Air, by the Hon.* Henry Cavendifh, *F. R. S.*

Read May 29, Nov. 6, and Nov. 13, 1766.

BY factitious air, I mean in general any kind of air which is contained in other bodies in an unelaftic ftate, and is produced from thence by art.

By fixed air, I mean that particular fpecies of factitious air, which is feparated from alcaline fubftances by folution in acids or by calcination; and to which Dr. Black has given that name in his treatife on quicklime.

As fixed air makes a confiderable part of the fubject of the following papers; and as the name might incline one to think, that it fignified any fort of air which is contained in other bodies in an unelaftic form; I thought it beft to give this explanation before I went any farther.

Before

Before I proceed to the experiments themſelves, it will be proper to mention the principal methods uſed in making them.

In order to fill a bottle with the air diſcharged from metals or alcaline ſubſtances by ſolution in acids, or from animal or vegetable ſubſtances by fermentation, I make uſe of the contrivance repreſented in TAB. VII. Fig. 1. where A repreſents the bottle, in which the materials for producing air are placed; having a bent glaſs tube C ground into it, in the manner of a ſtopper. E repreſents a veſſel of water. D the bottle to receive the air, which is firſt filled with water, and then inverted into the veſſel of water, over the end of the bent tube. F*f* repreſents the ſtring, by which the bottle is ſuſpended. When I would meaſure the quantity of air, which is produced by any of theſe ſubſtances, I commonly do it by receiving the air in a bottle, which has diviſions marked on its ſides with a diamond, ſhewing the weight of water, which it requires to fill the bottle up to thoſe diviſions: but ſometimes I do it by making a mark on the ſide of the bottle in which I have received the air, anſwering to the ſurface of the water therein; and then, ſetting the upright, find how much water it requires to fill it up to that mark.

In order to transfer the air out of one bottle into another, the ſimpleſt way, and that which I have ofteneſt made uſe of, is that repreſented Fig. 2. where A is the bottle, into which the air is to be transferred: it is ſuppoſed to be filled with water and inverted into the veſſel of water DEFG, and ſuſpended there by a ſtring: the line DG is the ſurface of the water: B repreſents a tin funnel held under the mouth of the bottle: C repreſents the inverted bottle, out of which the

the air is to be transferred; the mouth of which is lifted up till the air runs out of it into the funnel, and from thence into the bottle A.

In order to transfer air out of a bottle into a bladder, the contrivance Fig. 3. is made uſe of. A is the bottle out of which the air is to be transferred, inverted into the veſſel of water FGHK: B is a bladder whoſe neck is tied faſt over the hollow piece of wood C*c*, ſo as to be air-tight. Into the piece of wood is run a bent pewter pipe D, and ſecured with lute*. The air is then preſſed out of the bladder as well as poſſible, and a bit of wax E ſtuck upon the other end of the pipe, ſo as to ſtop up the orifice. The pipe, with the wax upon it, is then run up into the inverted bottle, and the wax torn off by rubbing it againſt the ſides. By this means, the end of the pipe is introduced within the bottle, without ſuffering any water to get within it. Then, by letting the bottle deſcend, ſo as to be totally immerſed in the water, the air is forced into the bladder.

The weights uſed in the following experiments, are troy weights, 1 ounce containing 480 grains. By an ounce or grain meaſure, I mean ſuch a meaſure as contains one ounce or grain Troy of water.

* The lute uſed for this purpoſe, as well as in all the following experiments, is compoſed of almond powder, made into a paſte with glue, and beat a good deal with a heavy hammer. This is the ſtrongeſt and moſt convenient lute I know of. A tube may be cemented with it to the mouth of a bottle, ſo as not to ſuffer any air to eſcape at the joint, though the air within is compreſſed by the weight of ſeveral inches of water.

Experiments on Factitious Air.

PART I.

Containing Experiments on Inflammable Air.

I Know of only three metallic ſubſtances, namely, zinc, iron and tin, that generate inflammable air by ſolution in acids; and thoſe only by ſolution in the diluted vitriolic acid, or ſpirit of ſalt.

Zinc diſſolves with great rapidity in both theſe acids; and, unleſs they are very much diluted, generates a conſiderable heat. One ounce of zinc produces about 356 ounce meaſures of air: the quantity ſeems juſt the ſame whichſoever of theſe acids it is diſſolved in. Iron diſſolves readily in the diluted vitriolic acid, but not near ſo readily as zinc. One ounce of iron wire produces about 412 ounce meaſures of air: the quantity was juſt the ſame, whether the oil of vitriol was diluted with $1\frac{1}{2}$, or 7 times its weight of water: ſo that the quantity of air produced ſeems not at all to depend on the ſtrength of the acid.

Iron diſſolves but ſlowly in ſpirit of ſalt while cold: with the aſſiſtance of heat it diſſolves moderately faſt. The air produced thereby is inflammable; but I have not tried how much it produces.

Tin was found to diſſolve ſcarce at all in oil of vitriol diluted with an equal weight of water, while cold: with the aſſiſtance of a moderate heat it diſſolved ſlowly, and generated air, which was inflammable: the quantity was not aſcertained.

Tin diſſolves ſlowly in ſtrong ſpirit of ſalt while cold: with the aſſiſtance of heat it diſſolves moderately faſt.

faſt. One ounce of tinfoil yields 202 ounce meaſures of inflammable air.

Theſe experiments were made, when the thermometer was at 50° and the barometer at 30 inches.

All theſe three metallic ſubſtances diſſolve readily in the nitrous acid, and generate air; but the air is not at all inflammable. They alſo unite readily, with the aſſiſtance of heat, to the undiluted acid of vitriol; but very little of the ſalt, formed by their union with the acid, diſſolves in the fluid. They all unite to the acid with a conſiderable effervesſence, and diſcharge plenty of vapours, which ſmell ſtrongly of the volatile ſulphureous acid, and which are not at all inflammable. Iron is not ſenſibly acted on by this acid, without the aſſiſtance of heat; but zinc and tin are in ſome meaſure acted on by it, while cold.

It ſeems likely from hence, that, when either of the above-mentioned metallic ſubſtances are diſſolved in ſpirit of ſalt, or the diluted vitriolic acid, their phlogiſton flies off, without having its nature changed by the acid, and forms the inflammable air; but that, when they are diſſolved in the nitrous acid, or united by heat to the vitriolic acid, their phlogiſton unites to part of the acid uſed for their ſolution, and flies off with it in fumes, the phlogiſton loſing its inflammable property by the union. The volatile ſulphureous fumes, produced by uniting theſe metallic ſubſtances by heat to the undiluted vitriolic acid, ſhew plainly, that in this caſe their phlogiſton unites to the acid; for it is well known, that the vitriolic ſulphureous acid conſiſts of the plain vitriolic acid

united to phlogiſton *. It is highly probable too, that the ſame thing happens in diſſolving theſe metallic ſubſtances in the nitrous acid; as the fumes produced during the ſolution appear plainly to conſiſt in great meaſure of the nitrous acid, and yet it appears, from their more penetrating ſmell and other reaſons, that the acid muſt have undergone ſome change in its nature, which can hardly be attributed to any thing elſe than its union with the phlogiſton. As to the inflammable air, produced by diſſolving theſe ſubſtances in ſpirit of ſalt or the diluted vitriolic acid, there is great reaſon to think, that it does not contain any of the acid in its compoſition; not only becauſe it ſeems to be juſt the ſame whichſoever of theſe acids it is produced by; but alſo becauſe there is an inflammable air, ſeemingly much of the ſame kind as this, produced from animal ſubſtances in putrefaction, and from vegetable ſubſtances in diſtillation, as will be ſhewen hereafter; though there can be no reaſon to ſuppoſe, that this kind of inflammable air owes its production to any acid. I now proceed to the experiments made on inflammable air.

I cannot find that this air has any tendency to loſe its elaſticity by keeping, or that it is at all abſorbed, either by water, or by fixed or volatile alcalies; as I have kept ſome by me for ſeveral weeks in a bottle inverted into a veſſel of water, without any ſenſible

* Sulphur is allowed by chymiſts, to conſiſt of the plain vitriolic acid united to phlogiſton. The volatile ſulphureous acid appears to conſiſt of the ſame acid united to a leſs proportion of phlogiſton than what is required to form ſulphur. A circumſtance which I think ſhews the truth of this, is that if oil of vitriol be diſtilled, from ſulphur, the liquor, which comes over, will be the volatile ſulphureous acid.

decreaſe of bulk; and as I have alſo kept ſome for a few days, in bottles inverted into veſſels of ſope leys and ſpirit of ſal ammoniac, without perceiving their bulk to be at all diminiſhed.

It has been obſerved by others, that, when a piece of lighted paper is applied to the mouth of a bottle, containing a mixture of inflammable and common air, the air takes fire, and goes off with an exploſion. In order to obſerve in what manner the effect varies according to the different proportions in which they are mixed, the following experiment was made.

Some of the inflammable air, produced by diſſolving zinc in diluted oil of vitriol, was mixed with common air in ſeveral different proportions, and the inflammability of theſe mixtures tried one after the other in this manner. A quart bottle was filled with one of theſe mixtures, in the manner repreſented in Fig. 2. The bottle was then taken out of the water, ſet upright on a table, and the flame of a lamp or piece of lighted paper applied to its mouth. But, in order to prevent the included air from mixing with the outward air, before the flame could be applied, the mouth of the bottle was covered, while under water, with a cap made of a piece of wood covered with a few folds of linnen; which cap was not removed till the inſtant that the flame was applied. The mixtures were all tried in the ſame bottle; and, as they were all ready prepared, before the inflammability of any of them was tried, the time elapſed between each trial was but ſmall: by which means I was better able to compare the loudneſs of the ſound in each trial. The reſult of the experiment is as follows.

With one part of inflammable air to 9 of common air, the mixture would not take fire, on applying the lighted paper to the mouth of the bottle; but, on putting it down into the belly of the bottle, the air took fire, but made very little ſound.

With 2 parts of inflammable to 8 of common air, it took fire immediately, on applying the flame to the mouth of the bottle, and went off with a moderately loud noiſe.

With 3 parts of inflammable air to 7 of common air, there was a very loud noiſe.

With 4 parts of inflammable to 6 of common air, the ſound ſeemed very little louder.

With equal quantities of inflammable and common air, the ſound ſeemed much the ſame. In the firſt of theſe trials, namely, that with one part of inflammable to 9 of common air, the mixture did not take fire all at once, on putting the lighted paper into the bottle; but one might perceive the flame to ſpread gradually through the bottle. In the three next trials, though they made an exploſion, yet I could not perceive any light within the bottle. In all probability, the flame ſpread ſo inſtantly through the bottle, and was ſo ſoon over, that it had not time to make any impreſſion on my eye. In the laſt mentioned trial, namely, that with equal quantities of inflammable and common air, a light was ſeen in the bottle, but which quickly ceaſed.

With 6 parts of inflammable to 4 of common air, the ſound was not very loud: the mixture continued burning a ſhort time in the bottle, after the ſound was over.

With

With 7 parts of inflammable to 3 of common air, there was a very gentle bounce or rather puff: it contined burning for ſome ſeconds in the belly of the bottle.

A mixture of 8 parts of inflammable to 2 of common air caught fire on applying the flame, but without any noiſe: it continued burning for ſome time in the neck of the bottle, and then went out, without the flame ever extending into the belly of the bottle.

It appears from theſe experiments, that this air, like other inflammable ſubſtances, cannot burn without the aſſiſtance of common air. It ſeems too, that, unleſs the mixture contains more common than inflammable air, the common air therein is not ſufficient to conſume the whole of the inflammable air; whereby part of the inflammable air remains, and burns by means of the common air, which ruſhes into the bottle after the exploſion.

In order to find whether there was any difference in point of inflammability between the air produced from different metals by different acids, five different ſorts of air, namely, 1. Some produced from zinc by diluted oil of vitriol, and which had been kept about a fortnight; 2. Some of the ſame kind of air freſh made; 3. Air produced from zinc by ſpirit of ſalt; 4. Air from iron by the vitriolic acid; 5. Air from tin by ſpirit of ſalt; were each mixed ſeparately with common air in the proportion of 2 parts of inflammable air to $7\frac{7}{10}$ of common air, and their inflammability tried in the ſame bottle, that was uſed for the former experiment, and with the ſame precautions. They each went off with a pretty loud noiſe, and without any difference in the ſound that I could

could be ſure of. Some more of each of the above parcels of air were then mixed with common air, in the proportion of 7 parts of inflammable air to $3\frac{1}{2}$ of common air, and tried in the ſame way as before. They each of them went off with a gentle bounce, and burnt ſome time in the bottle, without my being able to perceive any difference between them.

In order to avoid being hurt, in caſe the bottle ſhould burſt by the exploſion, I have commonly, in making theſe ſort of experiments, made uſe of an apparatus contrived in ſuch manner, that, by pulling a ſtring, I drew the flame of a lamp over the mouth of the bottle, and at the ſame time pulled off the cap, while I ſtood out of the reach of danger. I believe, however, that this precaution is not very neceſſary; as I have never known a bottle to burſt in any of the trials I have made.

The ſpecific gravity of each of the above-mentioned ſorts of inflammable air, except the firſt, was tried in the following manner. A bladder holding about 100 ounce meaſures was filledwith inflammable air, in the manner repreſented in Fig. 3. and the air preſſed out again as perfectly as poſſible. By this means the ſmall quantity of air remaining in the bladder was almoſt intirely of the inflammable kind. 80 ounce meaſures of the inflammable air, produced from zinc by the vitriolic acid, were then forced into the bladder in the ſame manner: after which, the pewter pipe was taken out of the wooden cap of the bladder, the orifice of the cap ſtopt up with a bit of lute, and the bladder weighed. A hole was then made in the lute, the air preſſed out as perfectly as poſſible, and the bladder weighed again. It was found to have increaſed

creaſed in weight $40\frac{3}{4}$ grains. Therefore the air preſſed out of the bladder weighs $40\frac{3}{4}$ grains leſs than an equal quantity of common air: but the quantity of air preſſed out of the bladder muſt be nearly the ſame as that which was forced into it, *i. e.* 80 ounce meaſures: conſequently 80 ounce meaſures of this ſort of inflammable air weigh $40\frac{3}{4}$ grains leſs than an equal bulk of common air. The three other ſorts of inflammable air were then tried in the ſame way, in the ſame bladder, immediately one after the other. In the trial with the air from zinc by ſpirit of ſalt, the bladder increaſed $40\frac{1}{2}$ grains on forcing out the air. In the trial with the air from iron, it increaſed $41\frac{1}{2}$ grains, and in that with the air from tin, it increaſed 41 grains. The heat of the air, when this experiment was made, was 50°; the barometer ſtood at $29\frac{3}{4}$ inches.

There ſeems no reaſon to imagine, from theſe experiments, that there is any difference in point of ſpecific gravity between theſe four ſorts of inflammable air; as the ſmall difference obſerved in theſe trials is in all probability leſs than what may ariſe from the unavoidable errors of the experiment. Taking a medium therefore of the different trials, 80 ounce meaſures of inflammable air weigh 41 grains leſs than an equal bulk of common air. Therefore, if the denſity of common air, at the time when this experiment was tried, was 800 times leſs than that of water, which, I imagine, muſt be near the truth *, inflam-

* Mr. Hawkſbee, whoſe determination is uſually followed as the moſt exact, makes air to be more than 850 times lighter than water; vid. Hawkſbee's experiments, p. 94, or Cotes's Hydroſtatics, p. 159. But his method of trying the experiment muſt in all probability make it appear lighter than it really is. For

mable air muſt be 5490 times lighter than water, or near 7 times lighter than common air. But if the denſity of common air was 850 times leſs than that of water, then would inflammable air be 9200 times

having weighed his bottle under water, both when full of air and when exhauſted, he ſuppoſes the difference of weight to be equal to the weight of the air exhauſted; whereas in reality it is not ſo much: for the bottle, when exhauſted, muſt neceſſarily be compreſſed, and on that account weigh heavier in water than it would otherwiſe do. Suppoſe, for example, that air is really 800 times lighter than water, and that the bottle is compreſſed $\frac{1}{12000}$ part of its bulk; which ſeems no improbable ſuppoſition: the weight of the bottle in water will thereby be increaſed by $\frac{1}{12000}$ of the weight of a quantity of water of the ſame bulk, or more than $\frac{1}{15}$ of the weight of the air exhauſted: whence the difference of weight will be not ſo much as $\frac{14}{15}$ of the weight of the air exhauſted: and therefore the air will appear lighter than it really is in the proportion of more than 15 to 14, *i. e.* more than 857 times lighter than water: whereas, if the ball had been weighed in air in both circumſtances, the error ariſing from the compreſſion would have been very trifling.

It appears, from ſome experiments that have been made by weighing a ball in air, while exhauſted, and alſo after the air was let in, that air, when the thermometer is at 50°, and the barometer at $29\frac{3}{4}$, is about 800 times lighter than water. Though the weight of the air exhauſted was little more than 50 grains, no error could well ariſe near ſufficient to make it agree with Hawkſbee's experiment. Air ſeems to expand about $\frac{1}{500}$ part by 1° of heat, whence its denſity in any other ſtate of the atmoſphere is eaſily determined. The denſity here aſſumed agrees very well with the rule given by the gentlemen, who meaſured the length of a degree in Peru, for finding the height of mountains barometrically, and which is given in the Connoiſſance des mouvemens celeſtes, année 1762. To make that rule agree accurately with obſervation, the denſity of air, whoſe heat is the ſame as that of the places where theſe obſervations were made, and which I imagine we may eſtimate at about 45°, ſhould be 798 times leſs than that of water, when the barometer ſtands at $29\frac{3}{4}$.

lighter than water, or $10\frac{8}{10}$ lighter than common air.

This method of finding the denſity of factitious air is very convenient and ſufficiently accurate, where the denſity of the air to be tried is not much leſs than that of common air, but cannot be much depended on in the preſent caſe, both on account of the uncertainty in the denſity of common air, and becauſe we cannot be certain but what ſome common air might be mixed with the inflammable air in the bladder, notwithſtanding the precautions uſed to prevent it; both which cauſes may produce a conſiderable error, where the denſity of the air to be tried is many times leſs than that of common air. For this reaſon, I made the following experiments.

I endeavoured to find the weight of the air diſcharged from a given quantity of zinc by ſolution in the vitriolic acid, in the manner repreſented in Fig. 4. A is a bottle filled near full with oil of vitriol diluted with about ſix times its weight of water: B is a glaſs tube fitted into its mouth, and ſecured with lute: C is a glaſs cylinder faſtened on the end of the tube, and ſecured alſo with lute. The cylinder has a ſmall hole at its upper end to let the inflammable air eſcape, and is filled with dry pearl-aſhes in coarſe powder. The whole apparatus, together with the zinc, which was intended to be put in, and the lute which was to be uſed in ſecuring the tube to the neck of the bottle, were firſt weighed carefully; its weight was 11930 grains. The zinc was then put in, and the tube put in its place. By this means, the inflammable air was made to paſs through the dry pearl-aſhes; whereby it muſt have been pretty effectually deprived of any acid

 or

or watery vapours that could have aſcended along with it. The uſe of the glaſs tube B was to collect the minute jets of liquor, that were thrown up by the effervefcence, and to prevent their touching the pearl-aſhes; for which reaſon, a ſmall ſpace was left between the glaſs-tube and the pearl-aſhes in the cylinder. When the zinc was diſſolved, the whole apparatus was weighed again, and was found to have loſt $11\frac{3}{4}$ grains in weight*; which loſs is principally owing to the weight of the inflammable air diſcharged. But it muſt be obſerved, that, before the effervefcence, that part of the bottle and cylinder, which was not occupied by other more ſolid matter, was filled with common air; whereas, after the effervefcence, it was filled with inflammable air; ſo that, upon that account alone, ſuppoſing no more inflammable air to be diſcharged than what was ſufficient to fill that ſpace, the weight of the apparatus would have been diminiſhed by the difference of the weight of that quantity of common air and inflammable air. The whole empty ſpace in the bottle and cylinder was about 980 grain meaſures, there is no need of exactneſs; and the difference of the weight of that quantity of common and inflammable air is about one grain: therefore the true weight of the inflammable air diſcharged, is $10\frac{3}{4}$ grains. The quantity of zinc uſed was 254 grains, and conſequently the weight of the air diſcharged is $\frac{1}{23}$ or $\frac{1}{24}$ of the weight of the zinc.

* As the quantity of lute uſed was but ſmall, and as this kind of lute does not loſe a great deal of its weight by being kept in a moderately dry room, no ſenſible error could ariſe from the drying of the lute during the experiment.

It

It was before ſaid, that one grain of zinc yielded 356 grain meaſures of air: therefore 254 grains of zinc yield 90427 grain meaſures of air; which we have juſt found to weigh $10\frac{3}{4}$ grains; therefore inflammable air is about 8410 times lighter than water, or $10\frac{1}{2}$ times lighter than common air.

The quantity of moiſture condenſed in the pearl-aſhes was found to be about $1\frac{1}{4}$ grains.

By another experiment, tried exactly in the ſame way, the denſity of inflammable air came out 8300 times leſs than than that of water.

The ſpecific gravity of the air, produced by diſſolving zinc in ſpirit of ſalt, was tried exactly in the ſame manner. 244 grains of zinc being diſſolved in ſpirit of ſalt diluted with about four times its weight of water, the loſs in effervеſcence was $10\frac{3}{4}$ grains; the empty ſpace in the bottle and cylinder was 914 grain meaſures; whence the weight of the inflammable air was $9\frac{3}{4}$ grains, and conſequently its denſity was 8910 times leſs than that of water.

By another experiment, its ſpecific gravity came out 9030 times lighter than water.

A like experiment was tried with iron. $250\frac{1}{2}$ grains of iron being diſſolved in oil of vitriol diluted with four times its weight of water, the loſs in effervеſcence was 13 grains, the empty ſpace 1420 grain meaſures. Therefore the weight of the inflammable air was $11\frac{3}{8}$ grains *i. e.* about $\frac{1}{22}$ of the weight of the iron, and its denſity was 8973 times leſs than that of water. The moiſture condenſed was $1\frac{1}{4}$ grains.

A like experiment was tried with tin. 607 grains of tinfoil being diſſolved in ſtrong ſpirit of ſalt, the loſs in effervеſcence was $14\frac{3}{4}$ grains, the empty ſpace 873

grain meaſures: therefore the weight of the inflammable air was $13\frac{3}{4}$ grains *i. e.* $\frac{1}{44}$ of the weight of the tin, and its denſity 8918 times leſs than that of water. The quantity of moiſture condenſed was about three grains.

It is evident, that the truth of theſe determinations depend on a ſuppoſition, that none of the inflammable air is abſorbed by the pearl-aſhes. In order to ſee whether this was the caſe or no, I diſſolved 86 grains of zinc in diluted acid of vitriol, and received the air in a meaſuring bottle in the common way. Immediately after, I diſſolved the ſame quantity of zinc in the ſame kind of acid, and made the air to paſs into the ſame meaſuring bottle, through a cylinder filled with dry pearl-aſhes, in the manner repreſented in Fig. 5. I could not perceive any difference in their bulks.

It appears from theſe experiments, that there is but little, if any, difference in point of denſity between the different ſorts of inflammable air. Whether the difference of denſity obſerved between the air procured from zinc, by the vitriolic and that by the marine acid is real, or whether it is only owing to the error of the experiment, I cannot pretend to ſay. By a medium of the experiments, inflammable air comes out 8760 times lighter than water, or eleven times lighter than common air.

In order to ſee whether inflammable air, in the ſtate in which it is, when contained in the inverted bottles, where it is in contact with water, contains any conſiderable quantity of moiſture diſſolved in it, I forced 192 ounce meaſures of inflammable air, through a cylinder filled with dry pearl-aſhes, by means of the ſame apparatus, which I uſed for filling the bladders with inflam-

inflammable air, and which is reprefented in Fig. 3. The cylinder was weighed carefully before and after the air was forced through; whereby it was found to have increafed 1 grain in weight. The empty fpace in the cylinder was 248 grains, the difference of weight of which quantity of common and inflammable air is $\frac{1}{4}$ of a grain. Therefore the real quantity of moifture condenfed in the pearl-afhes is $1\frac{1}{4}$ grain. The weight of 192 ounce meafures of inflammable air deprived of its moifture appears from the former experiments to be $10\frac{1}{2}$ grains; therefore its weight when faturated with moifture would be $11\frac{3}{4}$ grains. Therefore inflammable air, in that ftate in which it is in, when kept under the inverted bottles, contains near $\frac{1}{9}$ its weight of moifture; and its fpecific gravity in that ftate is 7840 times lefs than that of water.

I made an experiment with defign to fee, whether copper produced any inflammable air by folution in fpirit of falt. I could not procure any inflammable air thereby: but the phenomena attending it feem remarkable enough to deferve mentioning. The apparatus ufed for this experiment was of the fame kind as that reprefented in Fig. 1. The bottle A was filled almoft full of ftrong fpirit of falt, with fome fine copper wire in it. The wire feemed not at all acted on by the acid, while cold; but, with the affiftance of a heat almoft fufficient to make the acid boil, it made a confiderable effervefcence, and the air paffed through the bent tube, into the bottle D, pretty faft, till the air forced into it by this means feemed almoft equal to the empty fpace in the bent tube and the bottle A: when, on a fudden, without any fenfible alteration of the heat, the water rufhed violently

through the bent tube into the bottle A, and filled it almoſt intirely full.

The experiment was repeated again in the ſame manner, except that I took away the bottle D, and let out ſome of the water of the ciſtern: ſo that the end of the bent tube was out of water. As ſoon as the effervеſcence began, the vapours iſſued viſibly out of the bent tube; but they were not at all inflammable, as appeared by applying a piece of lighted paper to the end of the tube. A ſmall empty phial was then inverted over the end of the bent tube, ſo that the mouth of the phial was immerſed in the water, the end of the tube being within the body of the phial and out of water. The common air was by degrees expelled out of the phial, and its room occupied by the vapours; after which, having chanced to ſhake the inverted phial a little, the water ſuddenly ruſhed in, and filled it almoſt full; from thence it paſſed through the bent tube into the bottle A, and filled it quite full. It appears likely from hence that copper, by ſolution in the marine acid, produces an elaſtic fluid, which retains its elaſticity as long as there is a barrier of common air between it and the water, but which immediately loſes its elaſticity, as ſoon as it comes in contact with the water. In the firſt experiment, as long as any conſiderable quantity of common air was left in the bottle containing the copper and acid, the vapours, which paſſed through the bent tube, muſt have contained a good deal of common air. As ſoon therefore as any part of theſe vapours came to the farther end of the bent tube, where they were in contact with the water, that part of them, which conſiſted of the air from copper, would be immediately condenſed, leav-

ing the common air unchanged; whereby the end of the tube would be filled with common air only; by which means the vapours, contained in the reſt of the tube and bottle A, ſeem to have been defended from the action of the water. But when almoſt all the common air was driven out of the bottle, then the proportion of common air contained in the vapours, which paſſed through the tube, ſeems to have been too ſmall to defend them from the action of the water. In the ſecond experiment, the narrow ſpace left between the neck of the inverted phial and the tube would anſwer much the ſame end, in defending the vapours within the inverted phial from the action of the water, as the bent tube in the firſt experiment did in defending the vapours within the bottle from the action of the water.

Experiments on Factitious Air.

PART II.

Containing Experiments on Fixed Air, or that Species of Factitious Air, which is produced from Alcaline Subſtances, by Solution in Acids or by Calcination.

Experiment I.

THE air produced, by diſſolving marble in ſpirit of ſalt, was caught in an inverted bottle of water, in the uſual manner. In leſs than a day's time, much the greateſt part of the air was found to be abſorbed. The water contained in the inverted bottle was found to precipitate the earth from lime-water; a ſure ſign that it had abſorbed fixed air *.

* Lime, as Dr. Black has ſhewn, is no more than a calcarious earth rendered ſoluble in water by being deprived of its fixed

EXPERIMENT II.

I filled a Florence flaſk in the ſame way with the ſame kind of fixed air. When full, I ſtopt up the mouth of the flaſk with my finger, while under water, and removed it into a veſſel of quickſilver, ſo that the mouth of the flaſk was intirely immerſed therein. It was kept in this ſituation upwards of a week. The quickſilver roſe and fell in the neck of the flaſk, according to the alterations of heat and cold, and of the height of the barometer; as it would have done if it had been filled with common air. But it appeared, by comparing together the heights of the quickſilver at the ſame temper of the atmoſphere, that no part of the fixed air had been abſorbed or loſt its elaſticity. The flaſk was then removed, in the ſame manner as before, into a veſſel of ſope leys. The fixed air, by this means, coming in contact with the ſope leys, was quickly abſorbed.

I alſo filled another Florence flaſk with fixed air, and kept it with its mouth immerſed in a veſſel of quickſilver in the ſame manner as the other, for upwards of a year, without being able to perceive any air to be abſorbed. On removing it into a veſſel of ſope leys, the air was quickly abſorbed like the former.

It appears from this experiment, that fixed air has no diſpoſition to loſe its elaſticity, unleſs it meets with

air. Lime water is a ſolution of lime in water: therefore, on mixing lime water with any liquor containing fixed air, the lime abſorbs the air, becomes inſoluble in water, and is precipitated. This property of water, of abſorbing fixed air, and then making a precipitate with lime water, has been taken notice of by Mr. M'Bride.

water,

with water or ſome other ſubſtance proper to abſorb it, and that its nature is not altered by keeping.

EXPERIMENT III.

In order to find how much fixed air water would abſorb, the following experiment was made. A cylindrical glaſs, with diviſions marked on its ſides with a diamond, ſhewing the quantity of water which it required to fill it up to thoſe marks, was filled with quickſilver, and inverted into a glaſs filled with the ſame fluid. Some fixed air was then forced into this cylindrical glaſs, in the ſame manner that it was into the inverted bottles of water, in the former experiments; except that, to prevent any common air from being forced into the glaſs along with the fixed, I took care not to introduce the end of the bent tube within the cylindrical glaſs, till I was well aſſured that no common air to ſignify could remain within the bottle. This was done by firſt introducing the end of the bent tube within an inverted bottle of water, and letting it remain there, till the air driven into this bottle was at leaſt 10 times as much as would fill the empty ſpace in the bent tube, and the bottle containing the marble and acid. By this means one might be well aſſured, that the quantity of common air remaining within the bent tube and bottle muſt be very trifling. The end of the bent tube was then introduced within the cylindrical glaſs, and kept there till a ſufficient quantity of fixed air was let up. After letting it ſtand a few hours, the diviſion anſwering to the ſurface of the quickſilver in the cylinder was obſerved and wrote down, by which it was known how much fixed air had been let up. A little rain water

 was

was then introduced into the cylindrical glaſs, by pouring ſome rain water into the veſſel of quickſilver, and then lifting up the cylindrical glaſs ſo as to raiſe the bottom of it a little way out of the quickſilver. After having ſuffered it to ſtand a day or two, in which time the water ſeemed to have abſorbed as much fixed air as it was able to do, the diviſion anſwering to the upper ſurface of the water, and alſo that anſwering to the ſurface of the quickſilver, were obſerved: by which it was known how much air remained not abſorbed, and alſo how much water had been introduced: the diviſion anſwering to the ſurface of the water telling how much air remained not abſorbed, and the difference of the two diviſions telling how much water had been let up. More water was then let up in the ſame manner, at different times, till almoſt the whole of the fixed air was abſorbed. As all water contains a little air, the water uſed in this experiment was firſt well purged of it by boiling, and then introduced into the cylinder while hot. The reſult of the experiment is given in the following table; in which the firſt column ſhews the bulk of the water let up each time; the ſecond ſhews the bulk of air abſorbed each time; the third the whole bulk of water let up; the fourth the whole bulk of air abſorbed; and the fifth column ſhews the bulk of air remaining not abſorbed. In order to ſet the reſult in a clearer light, the whole bulk of air introduced into the cylinder is called 1, and the other quantities ſet down in decimals thereof.

Bulk of air let up =1.

Bulk of wate. let up each time.	Bulk of air abſorbed each time.	Whole bulk of water let up.	Whole bulk of air abſorbed.	Whole bulk of air remaining.
.322	.374	.322	.374	.626
.481	.485	.803	.859	.141
.082	.048	.885	.907	.093
.145	.079	1.030	.986	.014

I imagine that the quantities of water let up and of the air abſorbed could be eſtimated to about three or four 1000th parts of the whole bulk of air introduced. The height of the thermometer, during the trial of this experiment, was at a medium 55°.

This experiment was tried once before. The reſult agreed pretty nearly with this; but, as it was not tried ſo carefully, the reſult is not ſet down.

It appears from hence, that the fixed air contained in marble conſiſts of ſubſtances of different natures, part of it being more ſoluble in water than the reſt: it appears too, that water, when the thermometer is about 55°, will abſorb rather more than an equal bulk of the more ſoluble part of this air.

It appears, from an experiment which will be mentioned hereafter, that water abſorbs more fixed air in cold weather than warm; and, from the following experiment, it appears, that water heated to the boiling point is ſo far from abſorbing air, that it parts with what it has already abſorbed.

 EXPERI-

Experiment IV.

Some water, which had abſorbed a good deal of fixed air, and which made a conſiderable precipitate with lime water, was put into a phial, and kept about $\frac{1}{4}$ of an hour in boiling water. It was found when cold not to make any precipitate, or to become in the leaſt cloudy on mixing it with lime water.

Experiment V.

Water alſo parts with the fixed air, which it has abſorbed by being expoſed to the open air. Some of the ſame parcel of water, that was uſed for the laſt experiment, being expoſed to the air in a ſaucer for a few days, was found at the end of that time to make no clouds with lime water.

Experiment VI.

In like manner it was tried how much of the ſame ſort of fixed air was abſorbed by ſpirits of wine. The reſult is as follows.

Bulk of air introduced = 1.

Spirit let up each time.	Air abſorbed each time.	Whole bulk of ſpirit let up.	Whole bulk of air abſorbed.	Bulk of air remaining.
.207	.453	.207	.453	.547
.146	.274	.353	.727	.273
.074	.103	.427	.830	.170
.046	.030	.473	.860	.140

The

The mean height of the thermometer, during the trial of the experiment, was 46°. Therefore ſpirit of wine, at the heat of 46°, abſorbs near $2\frac{1}{4}$ times its bulk of the more ſoluble part of this air.

Experiment VII.

After the ſame manner it was tried how much fixed air is abſorbed by oil. Some olive oil, equal in bulk to $\frac{1}{3}$ part of the fixed air in the cylindrical glaſs, was let up. It abſorbed rather more than an equal bulk of air; the thermometer being between 40 and 50. The experiment was not carried any farther. The oil was found to abſorb the air very ſlowly.

Experiment VIII.

The ſpecific gravity of fixed air was tried by means of a bladder, in the ſame manner which was made uſe of for finding the ſpecific gravity of inflammable air; except that the air, inſtead of being caught in an inverted bottle of water, and thence transferred into the bladder, was thrown into the bladder immediately from the bottle which contained the marble and ſpirit of ſalt, by faſtening a glaſs tube to the wooden cap of the bladder, and luting that to the mouth of the bottle containing the efferveſcing mixture, in ſuch manner as to be air-tight. The bladder was kept on till it was quite full of fixed air: being then taken off and weighed, it was found to loſe 34 grains, by forcing out the air. The bladder was previouſly found to hold 100 ounce meaſures. Whence if the outward air, at the time when this experiment was tried, is ſuppoſed to have been 800 times lighter than water, fixed air is 511 times lighter than water, or $1\frac{57}{100}$ times heavier

heavier than common air. The heat of the air during the trial of this experiment was 45°.

By another experiment of the ſame kind, made when the thermometer was at 65°, fixed air ſeemed to be about 563 times lighter than water.

Experiment IX.

Fixed air has no power of keeping fire alive, as common air has; but, on the contrary, that property of common air is very much diminiſhed by the mixture of a ſmall quantity of fixed air; as appears from hence.

A ſmall wax candle burnt 80″ in a receiver, which held 190 ounce meaſures, when filled with common air only.

The ſame candle burnt 51″ in the ſame receiver, when filled with a mixture of one part of fixed air to 19 of common air, *i. e.* when the fixed air was $\frac{1}{20}$ of the whole mixture.

When the fixed air was $\frac{3}{40}$ of the whole mixture, the candle burnt 23″.

When the fixed air was $\frac{1}{10}$ of the whole, it burnt 11″.

When the fixed air was $\frac{6}{55}$ or $\frac{1}{9\frac{1}{6}}$ of the whole mixture, the candle went out immediately.

Hence it ſhould ſeem, that, when the air contains near $\frac{1}{9}$ its bulk of fixed air, it is unfit for ſmall candles to burn in. Perhaps indeed, if I had uſed a larger candle and a larger receiver, it might have burnt in a mixture containing a larger proportion of fixed air than this; as I believe that large flaming bodies will burn in a fouler air than ſmall ones. But this is ſufficient to ſhew, that the power, which common air has

has of keeping fire alive, is very much diminiſhed by a ſmall mixture of fixed air.

This experiment was tried, by ſetting the candle in a large ciſtern of water, in ſuch manner that the flame was raiſed but a little way above the ſurface; the receiver being inverted full of water into the ſame ciſtern. The proper quantity of fixed air was then let up, and the remaining ſpace filled with common air, by raiſing the receiver gradually out of water; after which, it was immediately whelmed gently over the burning candle.

Experiments on the Quantity of Fixed Air, contained in Alcaline Subſtances.

EXPERIMENT X.

The quantity of fixed air contained in marble was found by diſſolving ſome marble in ſpirit of ſalt, and finding the loſs of weight, which it ſuffered in efferveſcence, in the ſame manner as I found the weight of the inflammable air diſcharged from metals by ſolution in acids, except that the cylinder was filled with ſhreds of filtering paper inſtead of dry pearl aſhes; for pearl aſhes would have abſorbed the fixed air that paſſed through them. The weight of the marble diſſolved was $311\frac{1}{2}$ grains. The loſs of weight in efferveſcence was $125\frac{1}{2}$ grains. The whole empty ſpace in the bottle and cylinder was about 2700 grain meaſures: the exceſs of weight of that quantity of fixed, above an equal quantity of common, air is $1\frac{3}{4}$ grains. Therefore the weight of the fixed air diſcharged is $127\frac{1}{4}$ grains. The cylinder with the filtering paper was found to have increaſed $1\frac{3}{4}$ grains in weight during the efferveſcence. The empty ſpace in

in the cylinder was about 1160 grain meaſures: the exceſs of weight of which quantity of fixed air above an equal bulk of common air is $\frac{3}{4}$ grains. Therefore the quantity of moiſture condenſed in the filtering paper is one grain, or about $\frac{1}{125}$ part of the weight of the air diſcharged.

As water has been already ſhewn to abſorb fixed air, it ſeemed not improbable, but what there might be ſome fixed air contained in the ſolution of marble in ſpirit of ſalt; in which caſe the air diſcharged, during the effervеſcence, would not be the whole of the fixed air in the marble. In order to ſee whether this was the caſe, I poured ſome of the ſolution into lime water. It made ſcarce any precipitate; which, as the acid was intirely ſaturated with marble, it would certainly have done if the ſolution had contained any fixed air. It appears therefore from this experiment, firſt, that marble contains $\frac{127\frac{1}{4}}{311\frac{1}{2}} = \frac{407}{1000}$ of its weight of fixed air; and ſecondly, that the quantity of moiſture, which flies off along with the fixed air in effervеſcence, is but trifling; as I imagine that the greateſt part of what did fly off muſt have been condenſed in the filtering paper.

By another experiment tried much in the ſame way, marble was found to contain $\frac{408}{1000}$ of its weight of fixed air.

Experiment XI.

Volatile ſal ammoniac diſſolves with too great rapidity in acids, and makes too violent an effer-veſcence, to allow one to try what quantity of fixed air

air it contains in the foregoing manner: I therefore made uſe of the following method.

Three ſmall phials were weighed together in the ſame ſcale. The firſt contained ſome weak ſpirit of ſalt, the ſecond contained ſome volatile ſal ammoniac in moderate ſized lumps without powder, corked up to prevent evaporation, and the third, intended for mixing the acid and alcali in, contained only a little water, and was covered with a paper cap, to prevent the ſmall jets of liquor, which are thrown up during the effervefcence, from eſcaping out of the bottle. In order to prevent too violent an effervefcence, the acid and alcali were both added by a little at a time, care being taken that the acid ſhould always predominate in the mixture. Care was alſo taken always to cover the bottle with the paper cap, as ſoon as any of the acid or alcali were added. As ſoon as the mixture was finiſhed, the three phials were weighed again; whereby the loſs in effervefcence was found to be 134 grains. The weight of the volatile ſalt made uſe of was 254 grains, and was pretty exactly ſufficient to ſaturate the acid. The ſolution appeared, by pouring ſome of it into lime water, to contain ſcarce any fixed air. Therefore 254 grains of the volatile ſal ammoniac contain 134 grains of fixed air, *i.e.* $\frac{528}{1000}$ of their weight. It appeared from the ſame experiment, that 1680 grains of the volatile ſalt ſaturate as much acid as 1000 grains of marble.

By another experiment, tried with ſome of the ſame parcel of volatile ſalt, it was found to contain $\frac{538}{1000}$ of its weight of fixed air, and 1643 grains of it ſaturated as much acid as 1000 grains of marble. By a medium, the ſalt contained $\frac{533}{1000}$ of its weight

 of

of fixed air; and 1661 grains of it ſaturated as much acid as 1000 grains of marble.

One thouſand grains of marble were found to contain $407\frac{1}{2}$ grains of air, and 1661 grains of volatile ſal ammoniac contain 885 grains. Therefore this parcel of volatile ſal ammoniac contains more fixed air, in proportion to the quantity of acid that it can ſaturate, than marble does, in the proportion of 885 to $407\frac{1}{2}$, or of 217 to 100.

N.B. It is not unlikely, that the quantity of fixed air may be found to differ conſiderably in different parcels of volatile ſal ammoniac; ſo that any one, who was to repeat theſe experiments, ought not to be ſurprized if he was to find the reſult to differ conſiderably from that here laid down. The ſame thing may be ſaid of pearl aſhes.

Experiment XII.

This ſerves to account for a remarkable phenomenon, which I formerly met with, on putting a ſolution of volatile ſal ammoniac in water into a ſolution of chalk in ſpirit of ſalt. The earth was precipitated hereby, as might naturally be expected: but what ſurprized me, was, that it was attended with a conſiderable effervefcence; though I was well aſſured, that the acid in the ſolution of chalk was perfectly neutralized. This is very eaſily accounted for, from the above-mentioned circumſtance of volatile ſal ammoniac containing more fixed air in proportion to the quantity of acid that it can ſaturate, than calcareous earths do. For the volatile alcali, by uniting to the acid, was neceſſarily deprived of its fixed air. Part of this air united to the calcareous earth, which

was at the ſame time ſeparated from the acid; but, as the earth was not able to abſorb the whole of the fixed air, the remainder flew off in an elaſtic form, and thereby produced an effervefcence.

Experiment XIII.

The ſame ſolution of volatile ſal ammoniac made no precipitate, when mixed with a ſolution of Epſom ſalt; though a mixture thereof with a little ſpirit of ſal ammoniac, made with lime, immediately precipitated the magneſia from the ſame ſolution of Epſom ſalt; as it ought to do according to Dr. Black's account of the affinity of magneſia and volatile alcalies to acids. This experiment is not ſo eaſily accounted for as the laſt; but I imagine, that the magneſia is really ſeparated from the acid by the volatile alcali; but that it is ſoluble in water, when united to ſo great a proportion of fixed air, as is contained in a portion of volatile ſal ammoniac, ſufficient to ſaturate the ſame quantity of acid. The reaſon, why the mixture of the ſolution of volatile ſal ammoniac, with the ſpirits of ſal ammoniac made with lime, precipitates the magneſia from the Epſom ſalt, is that, as the ſpirits made with lime contain no fixed air, the mixture of theſe ſpirits with the ſolution of volatile ſal ammoniac contains leſs air in proportion to the quantity of acid which it can ſaturate, than the ſolution of volatile ſal ammoniac by itſelf does.

Volatile ſal ammoniac requires a great deal of water to diſſolve it, and the ſolution has not near ſo ſtrong a ſmell as the ſpirits of ſal ammoniac made with fixed alcali; the reaſon of which is, that the latter contain much leſs fixed air. But volatile ſal

ammoniac diſſolves in conſiderable quantity in weak ſpirits of ſal ammoniac made with lime, and the ſolution differs in no reſpect from the ſpirits made with fixed alcali. This is a convenient way of procuring the mild ſpirits of ſal ammoniac, as thoſe made with fixed alcali are ſeldom to be met with in the ſhops.

Experiment XIV.

The quantity of fixed air contained in pearl aſhes was tried, by mixing a ſolution of pearl aſhes with diluted oil of vitriol, in the ſame manner as was uſed for volatile ſal ammoniac. As much of the ſolution was uſed as contained $328\frac{1}{4}$ grains of dry pearl aſhes. The loſs of effervefcence was 90 grains. The mixture, which was perfectly neutralized, being then added to a ſufficient quantity of lime water, in order to ſee whether it contained any fixed air, a precipitate was made, which being dried weighed $8\frac{1}{2}$ grains. Therefore, if we ſuppoſe this precipitate to contain as much fixed air as an equal weight of marble, which I am well aſſured cannot differ very conſiderably from the truth, the fixed air therein is $3\frac{1}{2}$ grains, and conſequently the air in $328\frac{1}{4}$ grains of the pearl aſhes, is $93\frac{1}{2}$ grains, *i. e.* $\frac{284}{1000}$ of their weight.

By another experiment tried in the ſame way, they appeared to contain $\frac{287}{1000}$ of their weight of fixed air.

1558 grains of the pearl aſhes were found to ſaturate as much acid as 1000 grains of marble. Therefore this parcel of pearl aſhes contains more air in proportion to the quantity of acid that it can ſaturate, than marble does, in the proportion of 109 to 100.

Experiment XV.

Dr. Black ſays, that, by expoſing a ſolution of ſalt of tartar for a long time to the open air, ſome cryſtals were formed in it, which ſeemed to be nothing elſe than the vegetable alcali united to more than its uſual proportion of fixed air. This induced me to try, whether I could not perform the ſame thing more expeditiouſly, by furniſhing the alcali with fixed air artificially; which I did in the manner repreſented in Fig. 6: where A repreſents a wide-mouthed bottle, containing a ſolution of pearl aſhes; B*b* repreſents a round wooden ring faſtened over the mouth of the bottle, and ſecured with luting; C is a bladder bound tight over the wooden ring. This bladder, being firſt preſſed cloſe together, ſo as to drive out as much of the included air as poſſible, was filled with fixed air, by means of the bent tube D; one end of which is fixed into the wooden ring, and the other faſtened into the mouth of the bottle E, containing marble and ſpirit of ſalt. By this means the fixed air thrown into the bladder mixed with the air in the bottle, and came in contact with the fixed alcali. The fixed air was by degrees abſorbed, and cryſtals were formed on the ſurface of the fixed alcali, which were thrown to the bottom by ſhaking the bottle. When the alcali had abſorbed as much fixed air as it would readily do, the cryſtals were taken out and dryed on filtered paper, and the remaining ſolution evaporated; by which means ſome more cryſtals were procured.

N.B. It ſeemed, as, if not all the air diſcharged from the marble was of a nature proper to be abſorbed by the alcali, but only part of it; for when the alcali had abſorbed

abſorbed ſomewhat more than $\frac{1}{2}$ of the air firſt thrown into the bladder, it would not abſorb any more: but, on preſſing the remaining air out of the bladder, and ſupplying its place with freſh fixed air, a good deal of this new air was abſorbed. I cannot, however, ſpeak poſitively as to this point; as I am not certain whether the apparatus was perfectly air-tight*.

Theſe cryſtals do not in the leaſt attract the moiſture of the air; as I have kept ſome, during a whole winter, expoſed to the air in a room without a fire, without their growing at all moiſt or increaſing in weight.

Being held over the fire in a glaſs veſſel, they did not melt as many ſalts do, but rather grew white and calcined.

They diſſolve in about four times their weight of water when the weather is temperate, and diſſolve in greater quantity in hot water than cold.

It was found, by the ſame method, that was made uſe of for the volatile ſal ammoniac, that theſe cryſtals contain $\frac{423}{1000}$ of their weight of fixed air, and that 2035 grains of them ſaturate as much acid as 1000 grains of marble. Therefore theſe cryſtals contain more air in proportion to the quantity of acid they

* Pearl aſhes deprived of their fixed air, *i. e.* ſope leys, will abſorb the whole of the air diſcharged from marble; as I know by experience. But yet it is not improbable, but that the ſame alcali, when near ſaturated with fixed air, may be able to abſorb only ſome particular part of it. For as it has been already ſhewn, that part of the air diſcharged from marble is more ſoluble in water than the reſt; ſo it is not unlikely, but that part of it may have a greater affinity to fixed alcali, and be abſorbed by it in greater quantity than the reſt.

ſaturate,

saturate, than marble does, in the ratio of 211 to 100.

Experiment XVI.

As these crystals contain about as much fixed air in proportion to the quantity of acid, that they can saturate, as volatile sal ammoniac does, it was natural to expect, that they should produce the same effects with a solution of Epsom salt, or a solution of chalk in spirit of salt; as those effects seemed owing only to the great quantity of fixed air contained in volatile sal ammoniac. This was found to be the real case: for a solution of these crystals in five times their weight of water, being dropt into a solution of chalk in spirit of salt, the earth was precipitated, and an effervescence was produced. No precipitate was made on dropping some of the same solution into a solution of Epsom salt, though the mixture was kept upwards of twelve hours. But, upon heating this mixture over the fire, a great deal of air was discharged, and the magnesia was precipitated.

Experiments on Factitious Air.

PART III.

Containing Experiments on the Air, produced by Fermentation and Putrefaction.

MR. M'Bride has already shewn, that vegetable and animal substances yield fixed air by fermentation and putrefaction. The following experiments were made chiefly with a view of seeing, whether they yield any other sort of air besides that.

Experi-

Experiment I.

The air produced from brown ſugar and water, by fermentation, was caught in an inverted bottle of ſope leys in the uſual manner, and which is repreſented in Fig. 1. As the weather was too cold to ſuffer the ſugar and water to ferment freely, the bottle containing it was immerſed in water, which, by means of a lamp, was kept conſtantly at about 80° of heat. The quantity of ſugar put into the bottle was 931 grains: it was diſſolved in about $6\frac{1}{2}$ times its weight of water, and mixed with 100 grains of yeaſt, by way of ferment. The empty ſpace left in the fermenting bottle and tube together meaſured 1920 grains. The mixture fermented freely, and generated a great deal of air, which was forced up in bubbles into the inverted bottle, but was abſorbed by the ſope leys, as faſt as it roſe up. It frothed greatly; but none of the froth or liquor ran over. In about ten days, the fermentation ſeeming almoſt over, the veſſels were ſeparated. The bottle with the fermented liquor was found to weigh 412 grains leſs than it did, before the fermentation began. As none of the liquor ran over, and as little or no moiſture condenſed within the bent tube, I think one may be well aſſured, that the loſs of weight was owing intirely to the air forced into the inverted bottle; for the matter diſcharged, during the fermentation, muſt have conſiſted either of air, or of ſome other ſubſtance, changed into vapour: if this laſt was the caſe, I think it could hardly have failed, but that great part of thoſe vapours muſt have condenſed in the tube. The air remaining unabſorbed in the inverted bottle of ſope leys was meaſured, and was

 found

found to be exactly equal to the empty ſpace left in the bent tube and fermenting bottle. It appears therefore, that there is not the leaſt air of any kind diſcharged from the ſugar and water by fermentation, but what is abſorbed by the ſope leys, and which may therefore be reaſonably ſuppoſed to be fixed air. It ſeems alſo, that no part of the common air left in the fermenting bottle was abſorbed by the fermenting mixture, or ſuffered any change in its nature from thence: for a ſmall phial being filled with one part of this air, and two of inflammable air; the mixture went off with a bounce, on applying a piece of lighted paper to the mouth, with exactly the ſame appearances, as far as I could perceive, as when the phial was filled with the ſame quantities of common and inflammable air.

The ſugar uſed in this experiment was moiſt, and was found to loſe $\frac{228}{1000}$ parts of its weight by drying gently before a fire. Therefore the quantity of dry ſugar uſed was 715 grains; and the weight of the air diſcharged by fermentation appears to be near 412 grains, *i. e.* near $\frac{57}{100}$ parts of the weight of the dry ſugar in the mixture.

The fermented liquor was found to have intirely loſt its ſweetneſs; ſo that the vinous fermentation ſeemed to be compleated; but it was not grown at all ſour.

Experiment II.

The air, diſcharged from apple-juice by fermentation, was tried exactly in the ſame manner. The quantity ſet to ferment was 7060 grains, and was mixed with 100 grains of yeaſt. Some of the ſame parcel of

apple-juice, being evaporated gently to the confiftence of a moderately hard extract, was reduced to $\frac{1}{7}$ of its weight; fo that the quantity of extract, in the 7060 grains of juice employed, was 1009 grains. The liquor fermented much fafter than the fugar and water. The lofs of weight during the fermentation was 384 grains. The air remaining unabforbed in the inverted bottle of fope leys was loft by accident, fo that it could not be meafured; but, from the fpace it took up in the inverted bottle, I think I may be certain that it could not much exceed the empty fpace in the bent tube and fermenting bottle, if it did at all. Therefore there is no reafon to think that the apple-juice, any more than the fugar and water, produced any kind of air during the fermentation, except fixed air. It appears too, that the fixed air was near $\frac{381}{1000}$ of the weight of the extract contained in the apple-juice. The fermented liquor was very four; fo that it had gone beyond the vinous fermentation, and made fome progrefs in the acetous fermentation.

In order to compare more exactly the nature of the air produced from fugar by fermentation, with that produced from marble by folution in acids, I made the three following experiments.

Experiment III.

I firft tried in what quantity the air from fugar was abforbed by water, and at the fame time made a like experiment on the air difcharged from marble, by folution in fpirit of falt. This was done exactly in the fame way as the former experiments of this kind. The refult is as follows, beginning with the air from fugar and water.

Air

Air from ſugar and water let up =1000.

Bulk of water let up each time.	Bulk of air abſorbed each time.	Wholebulk of water let up.	Wholebulk of air abſorbed.	Bulk of air remaining.	Height of thermometer when obſerv. was made.
375	517	375	517	483	40
143	164	518	681	319	45
153	164	673	845	154	45
82	103	755	948	52	46

Air from marble let up =1000.

391	473	391	473	527	40
143	133	534	606	394	45
284	115	818	811	189	45
194	80.	1.012	891	109	46

The apparatus uſed in this experiment was ſuffered to remain in the ſame ſituation till ſummer, when the thermometer ſtood at 65°. The bulk of the air from ſugar, not abſorbed by the water, was then found to be 287; ſo that the matter had remitted 235 parts of air. The bulk of the air from marble not abſorbed, was 194; ſo that 85 parts were remitted; which is therefore a proof, that water abſorbs leſs fixed air in warm weather than cold.

It appears from this experiment, that the air produced from ſugar by fermentation, as well as that diſcharged from marble by ſolution in acids, conſiſts of ſubſtances of different nature: part being abſorbed by water in greater quantity than the reſt. But, in

 general,

general, the air from ſugar is abſorbed in greater quantity than that from marble.

In forcing the air from ſugar into the cylindrical glaſs, no ſenſible quantity of moiſture was found to condenſe on the ſurface of the quickſilver, or ſides of the glaſs; which is a proof that no conſiderable quantity of any thing except air could fly off from the ſugar and water in fermentation.

EXPERIMENT IV.

The ſpecific gravity of the air produced from ſugar was found in the ſame way as that produced from marble. A bladder holding 102 ounce meaſures, being filled with this kind of air, loſt $29\frac{1}{8}$ grains on forcing out the air, the thermometer ſtanding at 62°, and the barometer at $29\frac{1}{2}$ inches. Whence, ſuppoſing the outward air during the trial of this experiment to be 826 times lighter than water, as it ſhould be, according to the ſuppoſition made uſe of in the former parts of this paper, the air from ſugar ſhould be 554 times lighter than water. Its denſity therefore appears to be much the ſame as that of the air contained in marble; as that air appeared to be 511 times lighter than water, by a trial made when the thermometer was at 45°; and 563 times lighter, by another trial when the thermometer was at 65°.

This air ſeems alſo to poſſeſs the property of extinguiſhing flame, in much the ſame degree as that produced from marble; as appears from the following experiment.

EXPERIMENT V.

A ſmall wax candle burnt 15″ in a receiver filled with $\frac{1}{10}$ of air from ſugar, the reſt common air.

In a mixture containing $\frac{6}{55}$ or $\frac{1}{9\frac{1}{6}}$ of air from ſugar, the reſt common air, the candle went out immediately. When the receiver was filled with common air only, the ſame candle burnt 72″.

The receiver was the ſame as that uſed in the former experiment of this kind, and the experiment tried in the ſame way, except that the air from ſugar was firſt received in an empty bladder, and thence transferred into the inverted bottles of water, in which it was meaſured: for the air is produced from the ſugar ſo ſlowly, that, if it had been received in the inverted bottles immediately, it would have been abſorbed almoſt as faſt as it was generated.

It appears from theſe experiments, that the air produced from ſugar by fermentation, and in all probability that from all the other ſweet juices of vegetables, is of the ſame kind as that produced from marble by ſolution in acids, or at leaſt does not differ more from it than the different parts of that air do from each other, and may therefore juſtly be called fixed air. I now proceed to the air generated by putrefying animal ſubſtances.

Experiment VI.

The air produced from gravy broth by putrefaction, was forced into an inverted bottle of ſope leys, in the ſame way as in the former experiment. The quantity of broth uſed, was 7640 grains, and was found, by evaporating ſome of the ſame to the conſiſtence of a dry extract, to contain 163 grains of ſolid matter. The fermenting bottle was immerſed in water kept conſtantly to the heat of about 96°. In

about

about two days the fermentation ſeemed intirely over. The liquor ſmelt very putrid, and was found to have loſt $11\frac{1}{2}$ grains of its weight. The ſope leys had acquired a browniſh colour from the putrid vapours, and a muſty ſmell. The air forced into the inverted bottle, and not abſorbed by the ſope leys, meaſured 6280 grains: the air left in the bent tube and fermenting bottle was 1100 grains; almoſt all of which muſt have been forced into the inverted bottles: ſo that this unabſorbed air is a mixture of about one part of common air and $4\frac{7}{10}$ of factitious air.

This air was found to be inflammable; for a ſmall phial being filled with 109 grain meaſures of it, and 301 of common air, which comes to the ſame thing as 90 grains of pure factitious air, and 320 of common air, it took fire on applying a piece of lighted paper, and went off with a gentle bounce, of much the ſame degree of loudneſs as when the phial was filled with the laſt mentioned quantities of inflammable air from zinc and common air. When the phial was filled with 297 grains of this air, and 113 of common air, *i. e.* with 245 of pure factitious air, and 165 of common air, it went off with a gentle bounce on applying the lighted paper; but I think not ſo loud as when the phial was filled with the laſt-mentioned quantities of air from zinc and common air.

5500 grain meaſures of this air, *i. e.* 4540 of pure factitious air, and 960 of common air, were forced into a piece of ox-gut furniſhed with a ſmall braſs cock, which I find more convenient for trying the ſpecific gravity of ſmall quantities of air, than a bladder: the gut increaſed $4\frac{1}{2}$ grains in weight on

2 forcing

forcing out the air. A mixture of 4540 grains of air from zinc and 960 of common air being then forced into the ſame gut, it increaſed $4\frac{3}{4}$ grains on forcing out the air. So that this factitious air ſhould ſeem to be rather heavier than air from zinc; but the quantity tried was too ſmall to afford any great degree of certainty.

N.B. The weight of 4540 grain meaſures of inflammable air, is $\frac{58}{100}$ grains, and the weight of the ſame quantity of common air is $5\frac{7}{10}$ grains.

On the whole it ſeems that this ſort of inflammable air is nearly of the ſame kind as that produced from metals. It ſhould ſeem, however, either to be not exactly the ſame, or elſe to be mixed with ſome air heavier than it, and which has in ſome degree the property of extinguiſhing flame, like fixed air.

The weight of the inflammable air diſcharged from the gravy appears to be about one grain, which is but a ſmall part of the loſs of weight which it ſuffered in putrefaction. Part of the remainder, according to Mr. M'Bride's experiments, muſt have been fixed air. But the colour and ſmell, communicated to the ſope leys, ſhew, that it muſt have diſcharged ſome other ſubſtance beſides fixed and inflammable air.

Raw meat alſo yields inflammable air by putrefaction, but not in near ſo great a quantity, in proportion to the loſs of weight which it ſuffers, as gravy does. Four ounces of raw meat mixed with water, and treated in the ſame manner as the gravy, loſt about 100 grains in putrefaction; but it yielded hardly more inflammable air than the gravy. This air

ſeemed

ſeemed of the ſame kind as the former; but, as the experiments were not tried ſo exactly, they are not ſet down.

I endeavoured to collect in the ſame manner the air diſcharged from bread and water by fermentation, but I could not get it to ferment, or yield any ſenſible quantity of air; though I added a little putrid gravy by way of ferment.

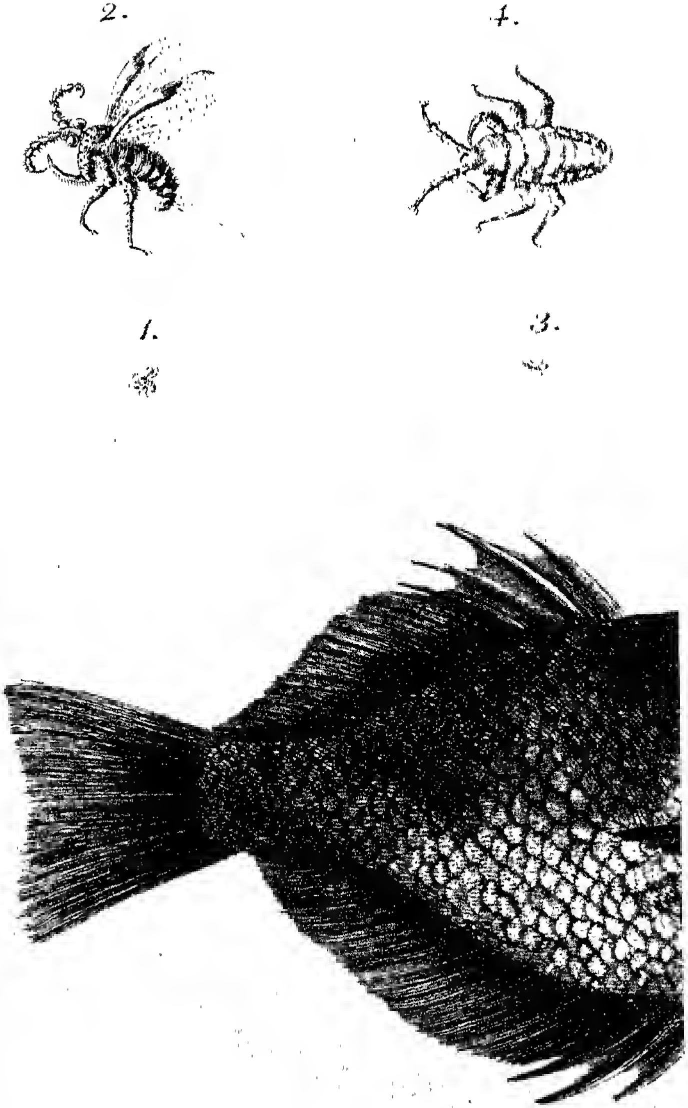
Received May 21, 1766.

XX. *A farther Account of the* Poliſh *Cochineal: from Dr.* Wolfe, *of* Warſaw. *Communicated by* Henry Baker, *F. R. S.*

Read June 5, 1766.

IN the LIVth volume of the Philoſophical Tranſactions, for the year 1764, Art. XV. the Royal Society has been pleaſed to publiſh two curious papers, communicated by Mr. Baker, from Dr. Wolfe of Warſaw, deſcribing the Poliſh Cochineal, the plants on whoſe roots it is found, the manner of collecting and curing it, the method of dying therewith, and alſo the doctor's own experiments on theſe curious inſects; the figures whereof are there given as engraven on a copper plate.

Since that time, the doctor has been very induſtrious in breeding and obſerving theſe inſects, and has thereby diſcovered the male fly, about which he was before uncertain; and has ſent to Mr. Baker an elegant picture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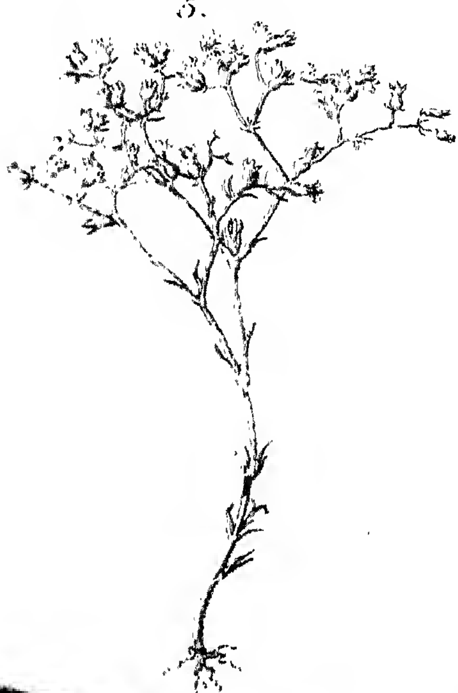

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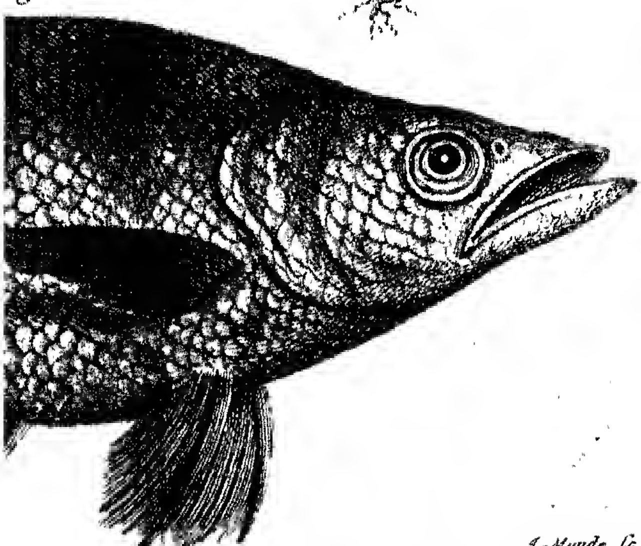


J. Mynde sc.

picture thereof, painted from the life in its natural colours, and also of the young female just crept from the egg, both in their natural size, and as magnified by glasses; together with a drawing and description of the polygonum minus of Casper Bauhine, or scleranthus perennis of Linnaeus, which is the plant, adhering to the roots of which, this insect is chiefly found in Podolia and the Ukrain.

All these Mr. Baker takes the liberty to lay before the Royal Society, to compleat Dr. Wolfe's account of this insect; and as this plant is common in England, as well as the Potentilla and Fragraria, at the roots of which these insects are likewise found, he is in hopes, that such gentlemen, as have opportunity, will seek them in the months of June, July and August; the time they seek for, and collect, them in the above-mentioned countries. The curious will receive pleasure and information from comparing the male fly of this Polish Cochineal, with the male fly of the Cochineal of South America; communicated some time ago by Mr. John Ellis, Fellow of this Society, and published in the Transactions, Vol. LII. p. 664.

Tab. VIII. Fig. 1. The Polish Cochineal male insect, just come out of the egg, of its natural size *.

Fig. 2. The same magnified.

Fig. 3. The female insect, just crept out of the egg, of the natural size. Its colour a brownish crimson.

Fig. 4. The same magnified.

Fig. 5. Polygonum minus IV^{tum} C. Bauh. or, Scleranthus perennis calycibus clausis, Linnaei.

* The body and head of this beautiful little fly have several teints of a brownish crimson: the wings are white and transparent, except the darker parts in the plate, which are of a lively crimson colour.

The root fibrous; when old, woody. The young ſtalks of a grey green; in the ſecond year, red. They have knots at different intervals. Each knot has two ſharp-pointed leafy narrow ſtipulae. The ſtalks are dichotomi: and near the umbella there is, at every bifurcation, a flower twice as big as the others, having its ſeeds more ripe and perfect.

The calyx grows almoſt woody, and is five-pointed. The petala are ſmall, oval-pointed, white, in number ten: the five ſtamina ſhort: the antherae yellow: the piſtilla two, very ſhort.

The ſeeds egg-ſhaped, one or two ſtrongly adhering to the calyx.

The whole plant, when old, has ſtalks ten inches in length, procumbent by the weight of the flowers, and making a ſort of convex buſh round about the root.

XXI. *Some further Intelligence relating to the Jaculator Fiſh, mentioned in the Philoſophical Tranſactions for* 1764, *Art. XIV. from Mr.* Hommel, *at* Batavia, *together with the Deſcription of another Species, by Dr.* Pallas, *F. R. S. in a letter to Mr.* Peter Collinſon, *F. R. S. from* John Albert Schloſſer, *M. D. F. R. S.*

Amſterdam, Feb. 15, 1766.

Read June 5, 1766.

WHEN the Jaculator fiſh intends to catch a fly or any other inſect, which is ſeen at a diſtance, it approaches very ſlowly and cautiouſly, and comes as much as poſſible perpendicularly

dicularly under the object: then the body being put in an oblique ſituation, more or leſs in this manner,  and the mouth and eyes being near the ſurface of the water, the Jaculator ſtays a moment quite immoveable, having its eyes directly fixed on the inſect, and then begins to ſhoot, without ever ſhewing its mouth above the ſurface of the water, out of which the ſingle drop, ſhot at the object, ſeems to riſe.

With the cloſeſt attention I never could ſee any part of the mouth out of water, though I have very often ſeen the Jaculator fiſh ſhoot a great many drops one after another, without leaving its place and fixed ſituation.

No more than two different ſpecies of this fiſh are found here.

The firſt and rareſt kind is that which I ſent before; and to the deſcription publiſhed in the LIVth volume of the Philoſophical Tranſactions, the foregoing account may be added. You now will receive from me, a ſpecimen of the ſecond ſpecies, which is the moſt common here.

Batavia, October 30,
1764.

Hommel.

Tab. VIII. Fig. 6. Sciæna Jaculatrix, quinque maculata, pinnis ventralibus adnatis, maxilla inferiore longiore.

Locus. Mare Indicum.

Magnitudo cyprini rutili.

Corpus

Corpus macrolepidotum (*covered with large ſcales*), dorſo convexo, verſus pinnam aſcendente, abdomine tumidulo, antice carinato.

Caput undique ſquamoſum, corpore craſſius, ſupra planiuſculum.

Os aſcendens, maxilla inferiore multo longiore, utriuſque marginibus ſubtiliter ſcabris.

Palatum oſſeum, concavo carinatum; ſubtiliſſime ſcabrum.

Lingua, plana, acuta, ſupra baſin in medio gibba.

Oculi magni, laterales, iride fulvo-aurea.

Pinnae pectorales, acuminatæ, radiorum XII.

Pinnae ventrales radiorum VI, primo breviore aculeato, triangulares, fere ſub pectoralibus, paulove poſterius, inter ſe membranula connexæ, quæ abdomini frœnulo adhaeret.

Squama lanceolata, frœnulo pinnarum ventralium utrinque accubans.

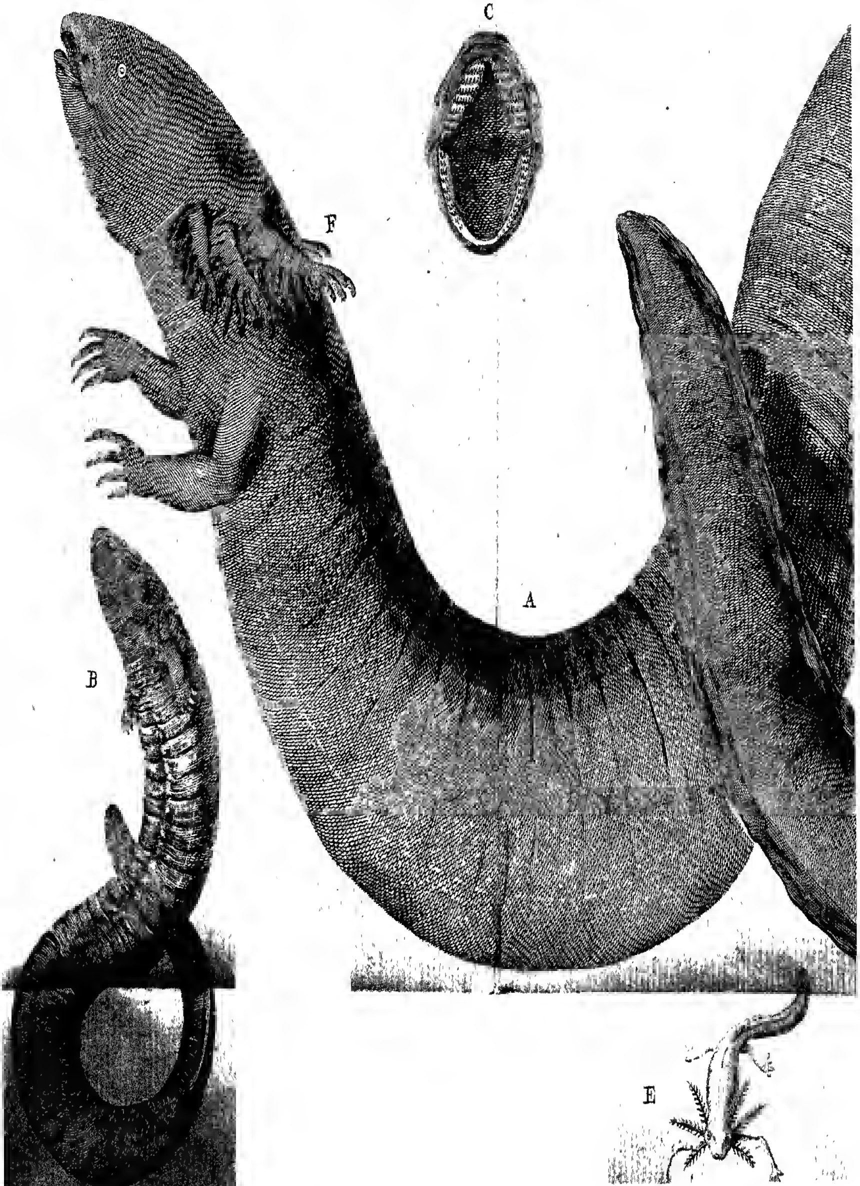
Pinna dorſalis dimidia dorſi longitudine, caudæ proxima, aſcendens baſi ſubcarnoſa, radiorum XIII in uno, XV in altero ſpecimine, quorum IV priores maximi, aculeati, primo breviore, membranis interſectis, unde pinna anterius ſerrata.

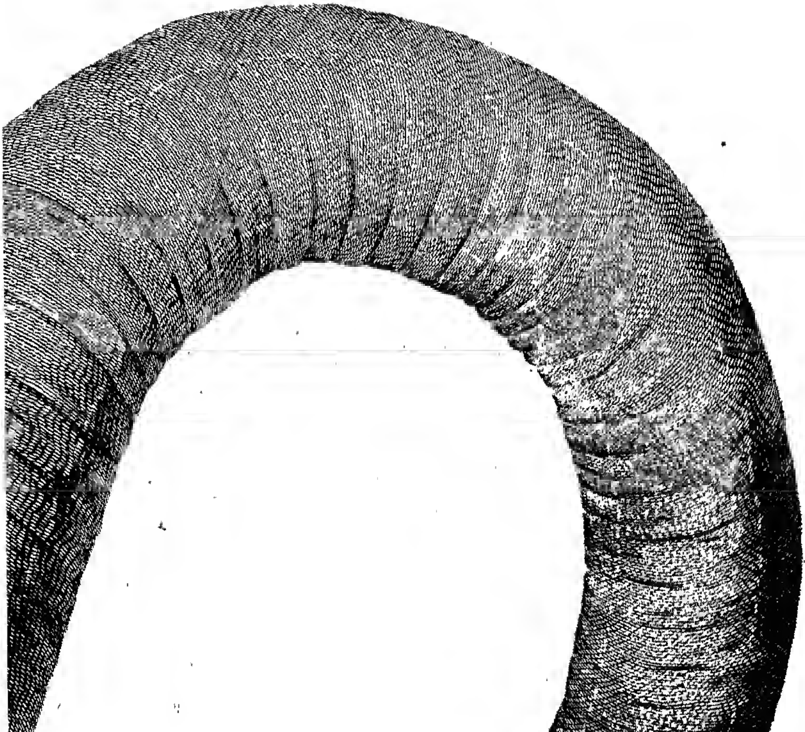
Pinna ani, dorſali oppoſita, baſi carnoſa, ſquamataque, ſeu carinæ caudæ productæ inſidens, radiorum XVIII, quorum tres priores aculeati.

Pinna caudæ ſubæqualis, radiorum XVII.

Color piſcis aureus, in dorſo fuceſcens; maculæ fuſcæ, tranſverſæ, ad dorſum utrinque quinæ, æquidiſtantes, prima ad tempora, ultima prope caudam.

SIREN *of Linnæus or Mud Inguana from S. Carolina*





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Received June 5, 1766.

XXII. *An Account of an Amphibious Bipes;* *by* John Ellis, *Esq;* *F. R. S.* *To the Royal Society.*

Read June 5, 1766.

THESE two ſpecimens of a remarkable kind of animal, which I have the honour to lay before this Royal Society, I received laſt ſummer from Dr. Alexander Garden, of Charles-town South Carolina, who ſays, it is evidently a new genus not yet taken notice of by naturaliſts, and that it appears to him, to come between the *Muræna* and the *Lacerta*.

The natives call it by the name of *Mud-Inguana.*

It is found in ſwampy and muddy places, by the ſides of pools, under the trunks of old trees that hang over the water.

Tab. IX. The leſſer one B, which is preſerved in ſpirits, meaſures about nine inches in length; and appears to be a very young ſtate of the animal, as we may obſerve from the fin of the tail and the opercula or coverings of the gills being not yet extended to their full ſize. Theſe opercula, in their preſent ſtate, conſiſt each of three indented lobes, hiding the gills from view, and are placed juſt above the two feet. Theſe feet appear like little arms and hands, each furniſhed with four fingers, and each finger with a claw.

In the ſpecimen A, which is about 31 inches long, the head is ſomething like an eel, but more compreſſed: the eyes are ſmall and placed as thoſe of the eel are, in this they are ſcarce viſible: this ſmallneſs of the eye.

eye beſt ſuits an animal that lives ſo much in mud. The noſtrils are very plainly to be diſtinguiſhed; theſe, with the gills and the remarkable length of the lungs, ſhew it to be a true amphibious animal. The mouth is ſmall in proportion to the body; but its palate and inſide of the lower jaw (ſee Fig. C) are well provided with many rows of pointed teeth; with this proviſion of nature, added to the ſharp exterior bony edges of both the upper and under jaw, the animal ſeems capable of biting and grinding the hardeſt kind of food. The ſkin, which is black, is full of ſmall ſcales, reſembling chagrin. Theſe ſcales are of different ſizes and ſhapes according to their ſituation, but all appear ſunk into its gelatinous ſurface: thoſe along the back and belly are of an oblong oval form, and cloſe ſet together: in the other parts, they are round and more diſtinct. Both the ſides are mottled with ſmall white ſpots, and have two diſtinct lines compoſed of ſmall white ſtreaks, continued along from the feet to the tail. The fin of the tail has no rays, and is no more than an adipoſe membrane like that of the eel; this fin appears more diſtinctly in the dry animal than in thoſe that have been preſerved in ſpirits.

The opercula or coverings to the gills in dry ſpecimens appear ſhrivelled up, but yet we may plainly ſee they have been doubly pennated. Under theſe coverings, are the openings to the gills, three on each ſide, agreeable to the number of the opercula. In the plate at Fig. F, the fins are repreſented as they appear when juſt taken out of the water and put into ſpirits of wne.

The form of theſe pennated coverings approach very near to what I have ſome time ago obſerved, in the

the larva or aquatic ſtate of our Engliſh lacerta, known by the name of eft or newt (ſee Fig. D and E) which ſerve them for coverings to their gills, and for fins to ſwim with during this ſtate; and which they loſe, as well as the fin of their tails, when they change their ſtate and become land animals; as I have obſerved by keeping them alive for ſome time myſelf.

Recollecting theſe obſervations on the changes of our lizard, and at the ſame time the many remarkable changes in frogs, I began to ſuſpect whether this animal might not be the larva ſtate of ſome large kind of lizard; and therefore requeſted the favour of Dr. Solander, to examine with me the lacertas in the Britiſh Muſeum; that we might ſee whether any of the young ones had only two feet; but, after carefully going through many kinds, we could plainly diſcover four feet perfectly formed, even in thoſe that were juſt coming out of their eggs.

During this ſtate of uncertainty, I forwarded to Dr. Linnæus of Upſal, at Dr. Garden's requeſt, his account of the largeſt ſpecimen, and, at the ſame time, ſent him one of the ſmaller ſpecimens preſerved in ſpirits; deſiring his opinion, for Dr. Garden's, as well as my own, ſatisfaction.

About the latter end of January laſt, I was favoured with an anſwer from the Profeſſor, dated Upſal, December 27, 1765, wherein he ſays,

" I received Dr. Garden's very rare two-footed ani-
" mal with gills and lungs. The animal is probably
" the larva of ſome kind of lacerta, which I very
" much deſire that he will particularly enquire into.

" If it does not undergo a change, it belongs to
" the order of *Nantes*, which have both lungs and gills;
" and

"and if fo, it muft be a new and very diftinct genus, "and fhould moft properly have the name of *Siren*.

"I cannot poffibly defcribe to you how much this "two-footed animal has exercifed my thoughts; if it "is a larva, he will no doubt find fome of them with "four feet.

"It is not an eafy matter to reconcile it to the larva "of the lizard tribe, its fingers being furnifhed with "claws; all the larvas of lizards, that I know, are "without them *(digitis muticis)*.

"Then alfo the branchiæ or gills are not to be "met with in the aquatic falamanders, which are "probably the larvas of lizards.

"Further, the croaking noife or found it makes "does not agree with the larvas of thefe animals; "nor does the fituation of the anus.

"So that there is no creature that ever I faw, that I "long fo much to be convinced of the truth, as what "this will certainly turn out to be."

I am, with the greateft refpect,
the Royal Society's
moft obedient humble fervant,

Gray's Inn,
June 5, 1766.

John Ellis.

P. S. In a letter lately received from Dr. Garden, he mentions one remarkable property in this animal, which is, that his fervant endeavouring to kill one of them, by dafhing it againft the ftones, it broke into three or four pieces: he further fays, that he has had an opportunity of feeing many of them lately of a much larger fize, and that he never faw one with more than two feet; fo that he is fully convinced, that it is quite a new genus of the animal kingdom.

Received

Received May 28, 1766.

XXIII. *Obſervations upon Animals, commonly called Amphibious by Authors. Preſented by Dr.* Parſons, *F. R. S.*

Read June 26, 1766.

THE following remarks, which I have the honour to lay before this learned Society, were occaſioned by a converſation that paſſed between me and a gentleman well acquainted with natural hiſtory, however miſtaken in the ſubject before us. His opinion was, that amphibious animals lived more in the water than on the land: but I believe the contrary will appear by the ſequel of this treatiſe.

If we conſider the words ἄμφι and βίος, from which the term amphibious is derived; we ſhould underſtand that animals, having this title, ſhould be capable of living as well by land or in the air, as by water, or of dwelling in either conſtantly at will; but it will be difficult to find any animal that can fulfil this definition, as being equally qualified for either; and in claſſing creatures of this kind, authors are much divided and ſometimes miſtaken.

Now if any natural hiſtorian ſhould deduce his diſtinction of this claſs, from the ſtructure or characteriſtic of any part of the animal, I think he would be a little out of the way; becauſe the term comprehends nothing but what regards its living in both air and water at diſcretion; however, ſince the word amphi-

amphibious is adopted by the writers of the hiſtory of animals, let us retain it ſtill, and examine ſome of this claſs, and, by conſidering their natural œconomy reſpectively, endeavour to range them according to that ſtandard in the following manner. They are ſuch as:

1. Enjoy their chief functions by land, but occaſionally go into the water.

2. Such as chiefly inhabit the water, but occaſionally go a ſhore. Of the latter there are but very few ſpecies. And although none of the winged tribe are to be ranged under this claſs, yet as many of them remain long upon the water, in ſearch of their proper food, we ſhall enumerate ſome peculiar advantages, which have been allowed to ſeveral of them by the bountiful wiſdom of the Creator, in order to render them the more able to obtain it; and this will make one curious part of my preſent purpoſe, not generally known.

The diſpute mentioned between my friend and me, turned upon the claſs of the phocæ, which conſiſts of a very numerous tribe of different ſpecies: I ſhall therefore endeavour to ſhew that none of them can live chiefly in the waters, but that their chief enjoyment of the functions of life is on ſhore.

Theſe animals are really quadrupeds; but, as their chief food is fiſh, they are under a neceſſity of going out to ſea to hunt their prey, and to great diſtances from ſhore; taking care that, how ever great the diſtance, rocks or ſmall iſlands are at hand, as reſting places when they are tired, or their bodies become too much macerated in the water; and they return to the places of their uſual reſort to ſleep, copulate, and

bring

bring forth their young, for the following reaſons, viz. It is well known that the only eſſential difference (as to the general ſtructure of the heart) between amphibious and meer land animals, or ſuch as never go into the water, is that in the former the oval hole remains always open. Now, in ſuch as are without this hole, if they were to be immerſed in water for but a little time, reſpiration would ceaſe, and the animal muſt die; becauſe a great part of the maſs of blood paſſes from the heart, by the pulmonary artery, through the lungs, and by the pulmonary veins returns to the heart; while the aorta is carrying the greater part of the maſs to the head and extremities, &c.

Now the blood paſſes through the lungs in a continual uninterrupted ſtream, while reſpiration is gentle and moderate; but when it is violent, then the circulation is interrupted, for inſpiration and expiration are now carried to their extent; and in this ſtate the blood cannot paſs through the lungs either during the total inſpiration or total expiration of the air in breathing; for in the former caſe the inflation compreſſes the returning veins, and in the latter, by the collapſion of the lungs, theſe veins are interrupted alſo, ſo that it is only between theſe two violent actions that the blood can paſs: and hence it is that the lives of animals are ſhortened, and their health impaired, when they are ſubjected to frequent violent reſpiration; and thus it is that in animals who have once breathed, they muſt continue to reſpire ever after; for life is at an end when that ceaſes.

There are three neceſſary and principal uſes of reſpiration in all land animals, and in theſe kinds that are counted amphibious; the firſt is that of promoting

the circulation of the blood through the whole body and extremities; in real fiſhes, the force of the heart is alone capable of ſending the blood to every part, as they are not furniſhed with limbs or extremities; but in the others mentioned, being all furniſhed with extremities, reſpiration is an aſſiſtant force to the arteries in ſending blood to the extremities, which, being ſo remote from the heart, have need of ſuch aſſiſtance; otherwiſe the circulation would be very languid in theſe parts; thus we ſee, that in perſons ſubject to aſthmatic complaints, the circulation grows languid, the legs grow cold and oedematous, and other parts ſuffer by the defect in reſpiration.

A ſecond uſe of breathing is that, in inſpiration, the variety of particles of different qualities, which float always in the air, might be drawn into the lungs, to be inſinuated into the maſs of blood, being highly neceſſary to contemperate and cool the agitated maſs, and to contribute refined pabulum to the finer parts of it, which, meeting with the daily ſupply of chyle, ſerves to aſſimilate and more intimately mix the maſs, and render its conſtitution the fitter for ſupporting the life of the animal. Therefore it is, that valetudinarians, by changing foul or unwholeſome air for a free, good, open air, often recover from lingring diſeaſes.

And a third principal uſe of reſpiration is, to promote the exhibition of a voice in animals; which all thoſe that live on the land do according to their ſpecific natures.

From theſe conſiderations it appears, beyond contradiction, that the phocæ of every kind are under an abſolute neceſſity of making the land their principal reſidence; but there is another very convincing argument

argument why they reside on shore the greatest part of their time, and that is, that the flesh of these creatures is analogous to that of other land animals; and therefore, by over-long maceration, added to the fatigue of their chacing their prey, they would suffer such a relaxation as would destroy them. It is well known that animals, which have lain long under water, are reduced to a very lax and even putrid state; and the phoca must bask in the air on shore; for while the solids are at rest, they acquire their former degree of tension, and the vigour of the animal is restored; and while he has an uninterrupted placid respiration, his blood is refreshed by the new supply of air, as I have explained it above, and he is rendered fit for his next cruise: for action wastes the most exalted fluids of the body, more or less, according to its duration and violence; and the restorative rest must continue a longer or shorter time, according to the quantity of the previous fatigue.

Let us now examine by what power these animals are capable of remaining longer under water than land animals.

All these have the oval hole open, between the right and left auricles of the heart, and, in many, the canalis arteriosus also: and while the phoca remains under water, which he may continue an hour or two more or less, his respiration is stopped, and the blood, not finding the passage through the pulmonary artery free, rushes through the hole from the right to the left auricle, and partly through the arterial canal, being a short passage to the aorta, and thence to every part of the body, maintaining the circulation: but, upon rising

to come aſhore, the blood finds its paſſage again through the lungs the moment he reſpires.

Thus the fœtus in utero, during his confinement, having the lungs compreſſed, and conſequently the pulmonary arteries and veins impervious, has the circulation of the blood carried on through the oval hole and the arterial canal; now ſo far the phoca in the water and the fœtus in utero are analogous; but they differ in other material circumſtances: one is, that the fœtus, having never reſpired, remains ſufficiently nouriſhed by the maternal blood circulating through him, and continues to grow till the time of his birth, without any want of reſpiration during nine months confinement; the phoca, having reſpired the moment of his birth, cannot live very long without it, for the reaſons given before; and this hole and canal would be cloſed in them, as it is in land animals, if the dam did not, very ſoon after the birth of the cub, carry him into the water to teach him, ſo very frequently; by which practice theſe paſſages are kept open during life; otherwiſe they would not be capable of attaining the food deſigned for them by providence.

Another difference is, that the phoca, as I ſaid before, would be relaxed by maceration in remaining too long in the water; whereas the fœtus in utero ſuffers no injury from continuing its full number of months in the fluid he ſwims in: the reaſon is; that water is a powerful ſolvent, and penetrates the pores of the ſkins of land animals, and in time can diſſolve them; whereas the liquor amnii is an inſipid ſoft fluid, impregnated with particles more or leſs mucilaginous, and utterly incapable of making the leaſt alteration in the cutis of the fœtus.

Otters, beavers, and ſome kinds of rats, go occaſionally into the waters for their prey, but cannot remain very long under water; I have often gone to ſhoot otters, and watched all their motions; I have ſeen one of them go ſoftly from a bank into the river, and dive down, and in about two minutes riſe, at ten or fifteen yards from the place he went in, with a midling ſalmon in his mouth, which he brought on ſhore; I ſhot him, and ſaved the fiſh whole. Now, as all fœtuſes have theſe paſſages open, if a whelp of a true water-ſpaniel was, immediately after its birth, ſerved as the phoca does her cubs, immerſed in water, to ſtop reſpiration for a little time every day, I make no doubt but the hole and canal would be kept open, and the dog be made capable of remaining as long under water as the phoca.

Frogs, how capable ſoever of remaining in the water, yet cannot avoid living on land, for they reſpire; and if, as I have often done, a frog be thrown into a river, he makes to the ſhore as faſt as he can.

The lizard kind, ſuch as may be called water lizards, or lacertæ aquaticæ, all are obliged to come to land and depoſite their eggs, reſt, and ſleep; even the crocodiles, who dwell much in rivers, ſleep and lay their eggs on ſhore; and, while in the water, are compelled to riſe to the ſurface to breathe; yet, from the texture of his ſcaley covering, he is capable of remaining in the water longer by far than any ſpecies of the phocæ, whoſe ſkin is analogous to that of a horſe or cow.

The hippopotamus, who wades into the lakes or rivers, is a quadruped, and remains under the water a conſiderable

confiderable time; yet his chief refidence is upon land, and he muft come on fhore for refpiration.

The teftudo, or fea-tortoife, though he goes out to fea, and is often found far from land; yet, being a refpiring animal, cannot remain long under water. He has indeed a power of rendering himfelf fpecifically heavier or lighter than the water, and therefore can let himfelf down to avoid an enemy or a ftorm; yet he is under a neceffity of rifing frequently to breathe, for reafons given before: and his moft ufual fituation, while at fea, is upon the furface of the water, feeding upon the various fubftances that float in great abundance every where about him; thefe animals fleep fecurely upon the furface, but not under water, and can remain longer at fea than any others of this clafs, except the crocodile, becaufe, as it is with the latter, his covering is not in danger of being too much macerated; yet they muft go on fhore to copulate and lay their eggs.

The confideration of thefe is fufficient to inform us of the nature of the firft order of the clafs of amphibious animals; let us now fee what is to be faid of the fecond in our divifion of them, which are fuch as chiefly inhabit the waters, but occafionally go on fhore.

Thefe are but of two kinds: the eels and water-ferpents, or fnakes of every kind. It is their form that qualifies them for loco-motion on land, and they know their way back to the water at will; for by their ftructure they have a ftrong periftaltic motion, by which they can go forward at a pretty good rate, whereas, all other kinds of fifh, whether vertical or horizontal, are incapable of a voluntary loco-motion on

ſhore; and therefore, as ſoon as ſuch fiſh are brought out of the water, after having flounced a while, they lye motionleſs, and ſoon die.

Let us now examine into the reaſon why theſe vermicular fiſh, the eel and ſerpent kinds, can live a conſiderable time on land, and the vertical and horizontal kinds die almoſt immediately, when taken out of the water: and, in this reſearch, we ſhall come to know what analogy there is between land animals and thoſe of the waters. All land animals have lungs, and can live no longer than while theſe are inflated by the ambient air, and alternately compreſſed for its expulſion; that is, while reſpiration is duly carried on, by a regular inſpiration and expiration of air.

In like manner, the fiſh in general have, inſtead of lungs, gills, or branchiæ; and, as in land animals, the lungs have a large portion of the maſs of blood circulating through them, which muſt be ſtopped if the air has not a free ingreſs and egreſs into and from them; ſo, in fiſh, there is a great ſhare of blood veſſels that paſs through the branchiæ, and a great portion of their blood circulates through them, which muſt in like manner be totally ſtopped, if the branchiæ are not kept perpetually wet with water; ſo that, as the air is to the lungs, in land animals, a conſtant aſſiſtant to the circulation, ſo is the water to the branchiæ of thoſe of the rivers and ſeas; for when theſe are out of the water, the branchiæ very ſoon grow criſp and dry, the blood veſſels are ſhrunk, and the blood is obſtructed in its paſſage; ſo, when the former are immerſed in water, or otherwiſe prevented having reſpiration, the circulation ceaſes, and the animal dies.

 Again,

Again, as land animals would be deſtroyed by too much maceration in water, ſo fiſhes would, on the other hand, be ruined by too much exſiccation; the latter being, from their general ſtructure and conſtitution, made fit to bear, and live in, the water; the former, by their conſtitution and forms, to breathe, and dwell, in the air.

But it may be aſked, why eels and water ſnakes are capable of living longer in the air than the other kinds of fiſh? this is anſwered, by conſidering the providential care of the great Creator for theſe and every one of his creatures: for, ſince they were capable of loco-motion by their form, which they need not be if they were never to go on ſhore, it ſeemed neceſſary that they ſhould be rendered capable of living a conſiderable time on ſhore, otherwiſe their loco-motion would be vain. How is this provided for? why in a moſt convenient manner; for this order of fiſhes have their branchiæ well covered from the external drying air, and are alſo furniſhed with a ſlimy mucus, which hinders their becoming criſp and dry for many hours, and their very ſkins always emit a mucous liquour, which keeps them ſupple and moiſt for a long time; whereas the branchiæ of other kinds of fiſh are much expoſed to the air, and want the ſlimy matter to keep them moiſt. Now, if, when any of theſe is brought out of the water, it was laid in a veſſel without water, he might be kept alive a conſiderable time, by only keeping the gills and ſurface of the ſkin conſtantly wet, even without any water to ſwim in.

Before I diſmiſs the firſt part of my diſcourſe, I muſt beg your patience, while I mention ſomething that relates to a family among the fiſh kinds, which is

of

of a middle nature between the phocæ, and the real fiſhes of the ſea, in one peculiar reſpect. This is the claſs of the phocenæ, or porpeſſes, of which there are ſeveral ſpecies; and theſe have lungs, and therefore are forced to come up to the ſurface to breathe at very ſhort intervals; but, when brought on ſhore, have no progreſſive loco-motion. So that, having lungs, they reſemble the phocæ, and, in every other reſpect, the real fiſhes of the ſea.

Blaſius, in his Anatome Animalium, page 288, gives an account of one of theſe taken and brought on ſhore alive; the people let him lye, to ſee how long he could live out of the water; and he continued alive only about ſeven or eight hours, and exhibited a kind of hiſſing voice.

From what has been ſaid, it will, I hope, appear rational, that theſe are the only two orders, that can properly be deduced from the claſs of Amphibious animals; and that the genus's of either order are very few in the animal world.

Received May 28, 1766.

XXIV. *An Account of ſome peculiar Advantages in the Structure of the Aſperæ Arteriæ, or Wind Pipes, of ſeveral Birds, and in the Land-Tortoiſe.*

Read June 9, 1766.

HAVING, in my former diſcourſe, given an account of ſome particular phœnomena, in amphibious animals, which rendered them more happy and perfect in their animal œconomy towards their preſervation; I ſhall now lay before this learned Society, certain advantages in ſome birds, towards aſſiſting them in the acquiſition of their food, which they ſeek for in the water; and ſome of theſe ſwim upon the water and dive down occaſionally; others only wade into the water, in ſhallow places, as far as their long legs will carry them, without touching the water with their feathers, in ſearch of their nouriſhment.

The natural hiſtory of four of them is very well ſet forth by authors; the other two are not mentioned, that I know of, but barely by their names; and, although the author has not deſcribed them, yet he knew the ſtructures of their aſperæ arteriæ, and was a perſon who made many obſervations in natural hiſtory, of whom I ſhall ſpeak in his turn.

Theſe birds are; the wild ſwan, colum, ſeras, crane, Indian cock, demoiſelle.

The ſtructure of the wind-pipe of all theſe is ſo ſingular and ſo little known, that I thought a proper notice of the ſubject would be agreeable to the Society, and very fit to be recorded together in the Tranſactions, if it be approved of (and will be a ſuitable ſequel to thoſe conſiderations upon amphibious animals); which I ſhall illuſtrate with drawings, to render their deſcription the more intelligible.

The wild ſwan is ſomewhat ſmaller than the tame one; this inhabits freſh rivers in land, while the wild one always reſorts great lakes and arms of the ſea. Theſe are two diſtinct ſpecies, the river ſwan, and the ſea or wild ſwan; and yet it has been ſuggeſted, that the latter might become as tame and familiar as the others, if they were brought up young: and hence they were ſuppoſed to be the ſame. But this wonderful ſtructure of the aſpera arteria ſhews that they are different, for the river ſwan has it not; although a very modern author, who is certainly as well verſed in natural hiſtory as any one whatever, has theſe remarkable words; " All the writers on birds; ſays he, " have deſcribed the ſwan: they have called it cygnus " domeſticus, and cygnus ferus, diſtinguiſhing it in " its wild and familiar ſtate into two ſpecies, but this " is idle and unneceſſary: the bird is wholly the " ſame in both."

It will be ſeen, however, by the deſcription of the part mentioned, they cannot be the ſame ſpecies; for, beſides this formation of the pipe, Mr. Edwards ſhews their heads to be very different alſo.

In the general run of birds, the aſpera arteria is nearly ſtrait; that is, having no plications, but deſcending directly from the epiglottis into the cavity of the

body

body to lye upon the ſternum, and terminating in the lungs; whereas, in theſe birds, which are the ſubjects of this diſcourſe, they have certain turnings within the ſternum or breaſt-bone, and run back again to double up into the thorax: which elongates them to double the length of thoſe in other birds of equal, nay of greater magnitude, than the birds that have them.

In the wild ſwan, the wind-pipe runs down from its upper extremity under the epiglottis, in company with the oeſophagus, till it comes within about four or five inches of the laſt vertebra of the neck; here the pipe quits the oeſophagus, (which keeps its courſe to the inteſtines) and makes a convex curve forward between the oſſa jugalia, in a circular ſweep, till it enters into a hole formed through a ſtrong membrane in the center between the inſertions of the oſſa jugalia into the ſternum under the breaſt; and, in that circular ſweep, is covered cloſely by the ſkin, ſo, that, in that place, a very ſlight blow would deſtroy the bird.

This hole is the beginning of a theca or cavity in the keel of the ſternum, in which the pipe paſſes on to the end, and then returns back, forming a loop which is circular; and, paſſing out by another hole through the ſame ſtrong membrane, makes another circular ſweep within, and parallel to the exterior one, and then riſes in that round direction, till it enters the cavity of the thorax, and is divided into two branchiæ, which terminate in the lungs.

When one views this ſtructure, it is impoſſible to avoid being ſurprized at the wonderful formation of this part, eſpecially too if we attend to the noble contri-

contrivance for ſecuring theſe circular volutions of the pipe, from compreſſing one another, or from bending into angles; for, if this was the caſe, their long and free reſpiration could not be maintained, and the end, for which the pipe is ſo formed, would not be anſwer-ed. An explanation of this contrivance will be ne-ceſſary in this place: there is a ſtrong membrane, which ariſes from all the clavicle, and is inſerted all along the jugal bone on each ſide, very ſtiff like a drum; and as the aſpera arteria makes its anterior volutions between the latter, (for the poſteriour turn-ing is that loop within the keel of the ſternum deſ-cribed,) it was neceſſary that the pipe ſhould be ſup-ported by a ſtay in each circular ſweep, to prevent the impediments juſt mentioned. Accordingly there are three ſtrong tranſverſe ligamentous membranes, run-ning from one jugal drum to the other; over the outer of which, the pipe goes into the keel of the ſternum through the under hole, and, in its return, rides over two others in a circular direction in its way to the thorax. Theſe are the ſtays, which prevent its doubling back in an angle, in theſe two volutions; and in that within the theca, there was no need of ſuch a fulcrum, being ſecured in its bed from any external preſſure.

This wild ſwan was brought alive from Philadelphia, but died ſoon after its arrival; and I aſſiſted in the diſſection, and made theſe drawings from the prepared parts. I find no mention of this ſtructure of the aſpera arteria in the wild ſwan*, but originally in

* Mr. Edwards found it in the ſwan he deſcribes. See his Hiſtory of Birds.

Bartholin,

Bartholin, who took delight in comparative anatomy, from whom Blaſius has taken it.

It is difficult to ſay what may be the real uſe of this kind of wind-pipe in the ſeveral birds that have it, if it be not to procure them a longer retention of inſpired air, (while they ſeek their food where they are obliged to remain ſome time immerſed in water,) than if the pipe was ſtrait, as in geeſe, ducks, and ſuch like; for theſe and the river ſwan often dive down to feed, yet it is always in ſhallow places, and their continuance under water is very ſhort; whereas the wild ſwan dwells upon and ſeeks his food in great lakes, and arms of the ſea; and dives into deeper waters, and conſequently requires a power of continuing longer without reſpiration than the others.

Poets and natural hiſtorians in great numbers have aſſerted that theſe birds ſing very harmoniouſly; and this gave occaſion to a friend of mine, to whom I ſhewed theſe drawings, to ſurmiſe that this ſtructure might be of uſe to them in ſinging; but I never found any one who would ſay they ever heard either wild or river ſwans ſing: and therefore I doubt it much. But, if they do ſing, the length of the pipe contributes nothing towards it; it is the glottis, which forms the voice, and modulates it, whether the pipe be long or ſhort: beſides, none of the ſong, or ſpeaking-birds, have any flection in their pipes, that we are acquainted with.

The crane is the next I ſhall take occaſion to mention, which has ſuch a turning of the aſpera arteria in the keel of the ſternum; but the volution of this bird is round within the bone, and may be compared to that of a French-horn; whereas that of the wild

ſwan

ſwan is ſtrait within the bone, and may be compared to a trumpet; yet the entrance of this into the ſternum, and its exit, and its paſſage into the cavity of the thorax, are ſimilar to thoſe of the ſwan.

This is a bird who cannot go upon the water, being no more capable of ſwimming than a common cock or hen. His feathers will not admit of it; and, having no webs to his toes, it is unable to ſwim. It is ſomewhat ſurpriſing that not one of the tribes which are ſimilar to the crane, ſuch as the herons, ſtorks, bitterns, &c. has any ſuch ſtructure of the aſpera arteria; and yet they all feed upon fiſh or water inſects. We are, however, to conſider, that the heron chiefly haunts brooks, ſprings, and the narrow heads of rivers, where he ſeeks his food, and finds it with eaſe: but the crane is under a neceſſity of immerſing its head, and remains a conſiderable ſpace in that ſituation upon ſtrands and marſhes: it is alſo a bird of congreſs; for, at certain ſeaſons, a multitude of cranes flock together and riſe upon the wing to a great height in the air, being birds of paſſage, and they are by many authors ſaid to travel from moſt parts of Scythia to Egypt, where, for a certain ſeaſon, they remain about the Nile, and the great lakes of that country. Perhaps this elongation of the windpipe in them may be alſo of uſe, in their great flight through various degrees of rarified or condenſed air, in the variety of climates through which they paſs.

The Indian cock, Gallus Indicus of Aldrovand and Longalius, and Gallus Perſicus of Johnſton, the Mutu Poranga of Margrave, is not the Coq d'Inde, or Turkey cock, but by the laſt author ranged among the pheaſant tribe; this bird has a plication of the aſpera

arteria, but not ſo conſiderable as either of the fore-mentioned, ſwan or crane: for it deſcends in a ſtrait line, along with the oeſophagus, to the middle of the jugal bone, without and above the thorax, where it is ſpread and faſtened on each ſide. Then, turning backwards, being ſomewhat flat, it makes a fold upward to about an inch and half high, and there being made faſt again, by a ſtrong membrane, it doubles down and paſſes into the thorax, terminating by two bronchia in the lungs: and where it is faſtened and folded, that is, in the flat parts, it is triple the circumference of any other part of the pipe. This bird and another of the ſame ſpecies were diſſected by the Royal Academy of Sciences, and this ſtructure of the pipe appeared in both; for which it is difficult to aſſign a reaſon in any of the pheaſant kinds, if it be not to retain inſpired air longer than ordinary upon ſome occaſions, though they are not frequenters of rivers or marſhy grounds; which one might reaſonably ſuggeſt from the great capacity in the plicated parts of the wind-pipe.

The next I ſhall mention is the Grus Numidica, Numidian crane, or Demoiſelle.

This bird has alſo a plication in the wind-pipe, which was likewiſe diſſected by the Academy of Sciences, in whoſe account the natural hiſtory of it may be ſeen; and a true deſcription and figure of it from the life, by Mr. Edwards, in his Natural hiſtory of birds. I confine myſelf only to the configuration of this part, in as many animals as I can find endowed with ſuch a ſtructure, in order to collect them and lay them here in view: and ſhall hereafter make farther reſearches and diſſections, in ſuch as I may reaſonably

ſuppoſe to have a different formation from the common ſtandard of a ſtrait wind-pipe.

In this bird, being of the crane kind, the pipe runs down in company with the oeſophagus, to about a foot in length, and then turns outwards and forwards, as it does in the ſwan and crane, and enters into the keel of the ſternum, which, like the others, forms a bony box for its reception, through a ligamentous hole for about three inches: then it returns upwards, and a round turn into the thorax terminating in bronchia and lungs.

Now, in the Indian cock mentioned, the plication is made above the ſternum, in a roomy part between the jugal bones; whereas, in the others mentioned, the plication is within the keel of the ſternum.

The other birds I find any account of, having the aſpera arteria folded, are only two, and of theſe our information is very ſhort.

In Dr. Fryers's account of India and Perſia, where he treats of his deſcription of Surat and his journey into Duccan, page 119, I find the following paſſage: "Fiſh, oiſters, ſoles, and Indian "mackerel, the river yields very good; and the pools "and lakes ſtore of wild fowl; particularly brand "geeſe, colum, and ſeraſs, a ſpecies of the former; "in the cold weather, they, ſhunning the northern "rigid blaſts, come yearly hither from mount Cauca-"ſus; what is worth taking notice of, is their aſpera "arteria wound up in a caſe on both ſides their breaſt-"bone, in manner of a trumpet, ſuch as our waits "uſe: when it is ſingle, it is a ſeraſs; when double, "it is a colum, making a greater noiſe than a

"bittern,

" bittern, being heard a great while before they can
" be ſeen, flying in armies in the air."

From this paſſage, it is plain our author had examined the interior parts of this colum and ſeraſs, and that they are different ſpecies of the ſame genus. We can only, however, endeavour to find what this genus is; and, by what we have heard of the crane, it is not improbable that they are of that kind. The crane, by every author, is ſaid to take long flights in vaſt multitudes; and to make a great noiſe in the air. The colum and ſeraſs are ſaid to come to the rivers and lakes about Surat or Duccan from Caucaſus, flying in armies, and making ſo great a noiſe, that they are heard a great while before they are ſeen. Again, by his ſhort account, we may eaſily learn, that they are not the crane that is deſcribed in this paper, becauſe the colum has a volution of the wind-pipe on both ſides of the keel of the ſternum, the crane but one; and I ſuppoſe, from the likeneſs of the ſeraſs to the colum, he ſays, that the former is a ſpecies of the latter; nor can we have room to ſuggeſt that theſe birds are of the wild gooſe kind; becauſe he mentions the brand geeſe firſt, without taking the leaſt notice of their aſpera arteria; and confines the reſt of the paragraph to the other two, calling the one a ſpecies of the other.

Theſe are all the birds that have hitherto come to my notice, having this remarkable flection in the aſpera arteria; it now only remains to mention farther that of the land tortoiſe, which I bring in here on account of a ſimilar formation of that organ in him.

Having never diſſected a land tortoiſe myſelf, I muſt have recourſe to thoſe that have; and acordingly I find

find its parts examined by a celebrated phyſician of his time, Velchſius, from whom Blaſius has taken it, and by the Academy of Sciences. Blaſius has, however, made the anatomical diſtribution of its parts and their explanation; from whom I ſhall take the figure in a ſomewhat larger ſcale, for its better illuſtration; and although this laſt author quotes Severinus and Coiterus as diſſectors of the tortoiſe, as well as Velchſius, yet this latter only mentions the wind-pipe.

In Blaſius's figure, this pipe, for a few inches from the epiglottis, is ſingle, but ſoon divides into two; and as it deſcends in company with the oeſophagus, it forms a folded ring outwards on each ſide, and turns down again to enter the lungs: ſo that the animal has the advantage of a double aſpera arteria, with a volution in each: which ſhews that this proviſion is intended to contain a greater portion of air than ordinary while he is under ground in winter.

The tortoiſe diſſected by the Academy of Sciences, was a large land tortoiſe from the coaſt of Coromandel; in which was found a biffid wind-pipe; each branch is ſaid to be ſix inches long, but no mention is made of the volutions in this land tortoiſe; which, one would think, being ſo remarkable a variety from the common ſtandard, ought not to eſcape the notice of ſuch able anatomiſts. Yet it was found, by ſeveral experiments, that reſpiration was very ſlow and unequal in this animal, as well as in the camelion; the Academicians obſerved ſeveral tortoiſes for a long time together, and have taken notice, that they ſometimes caſt forth a cold breath through the noſtrils, but that is by long intervals and without order; and that the camelion is ſome-

ſometimes half a day without one's being able to diſcern in him any motion for the reſpiration.

From this it is eaſily ſeen, that they can retain inſpired air a long time: and the Academicans therefore think, that the principal uſe of the lungs in tortoiſes, is to render themſelves ſpecifically lighter or heavier in the water, by their inflation and compreſſion at will, as fiſhes do by their ſwimming bladders; indeed ſuch a power of long inſpiration ſeems to be as neceſſary in the land tortoiſe as in that of the ſea; becauſe, in many countries where they breed, they are known to go into the ground and lye concealed for ſeveral months; and it is well known, that ſeveral ſpecies of land tortoiſes go into ponds or canals in gardens, where they are kept, and remain long under water at pleaſure. Of this my worthy friend Mr. Collinſon had inſtances in his gardens at his country ſeat: and I ſaw two land tortoiſes in the bottom of a circular canal, in the gardens of the Palais Royal in Paris ſeveral times, which were very large ones, and remained under water many hours together.

The ingenious Academicians, however, in order to verify their ſentiments, that one principal uſe of the lungs in a tortoiſe is to render them capable of remaining at any depth in the water at will, made the following experiment: they locked up a living tortoiſe in a veſſel of water intirely full; on which there was a cover exactly faſtened with wax, from which there went a glaſs pipe: the veſſel being full, ſo as to make the water appear at the bottom of the glaſs pipe; they obſerved the water did ſometimes aſcend into the pipe, and that it ſometimes deſcended. Now this could be done only by the augmentation and diminution

Fig. 1.

a. The Aspera Arteria.

b. The Oesophagus.

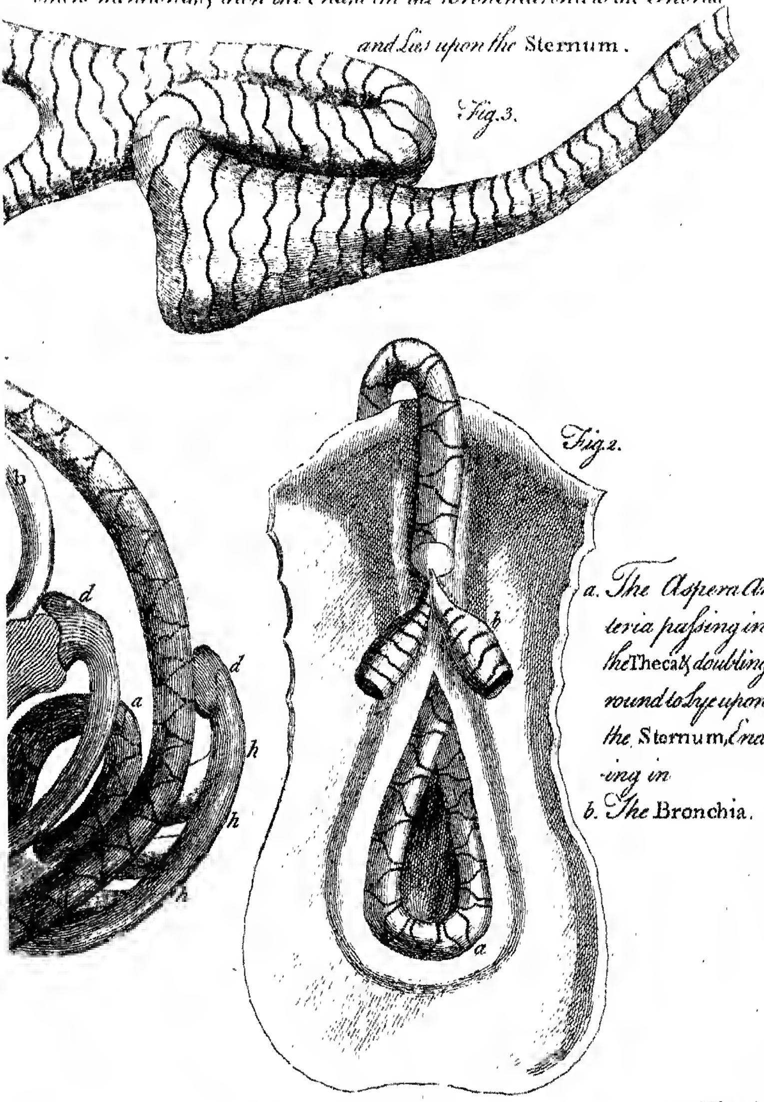
c. Vertebræ of the Neck.

d. The ossa Jugalia.

e. The Keel of the Sternum.

h. The Membranes that Support the Aspera Arteria.

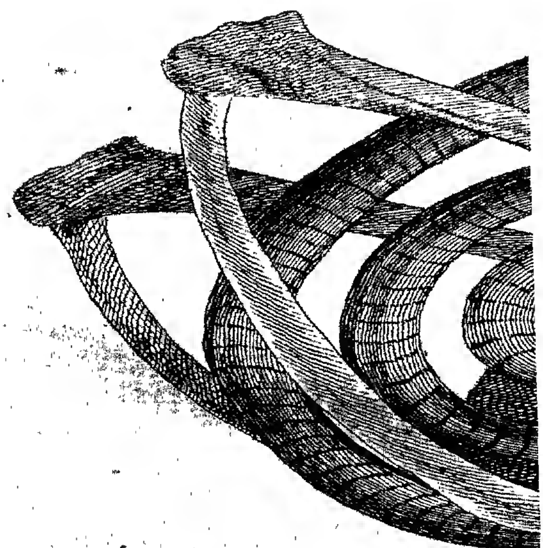
The Aspera Arteria of the Indian Cock. this Plication is made between the Jugal Bones & not in the Breast Bone, as are the others mention'd, & then the End, with the Bronchia, enters the Thorax and Lies upon the Sternum.



a. The Aspera Arteria passing in the Theca & doubling round to Lye upon the Sternum, Ending in
b. The Bronchia.

J. Mynde sc.

The Course of the Aspera



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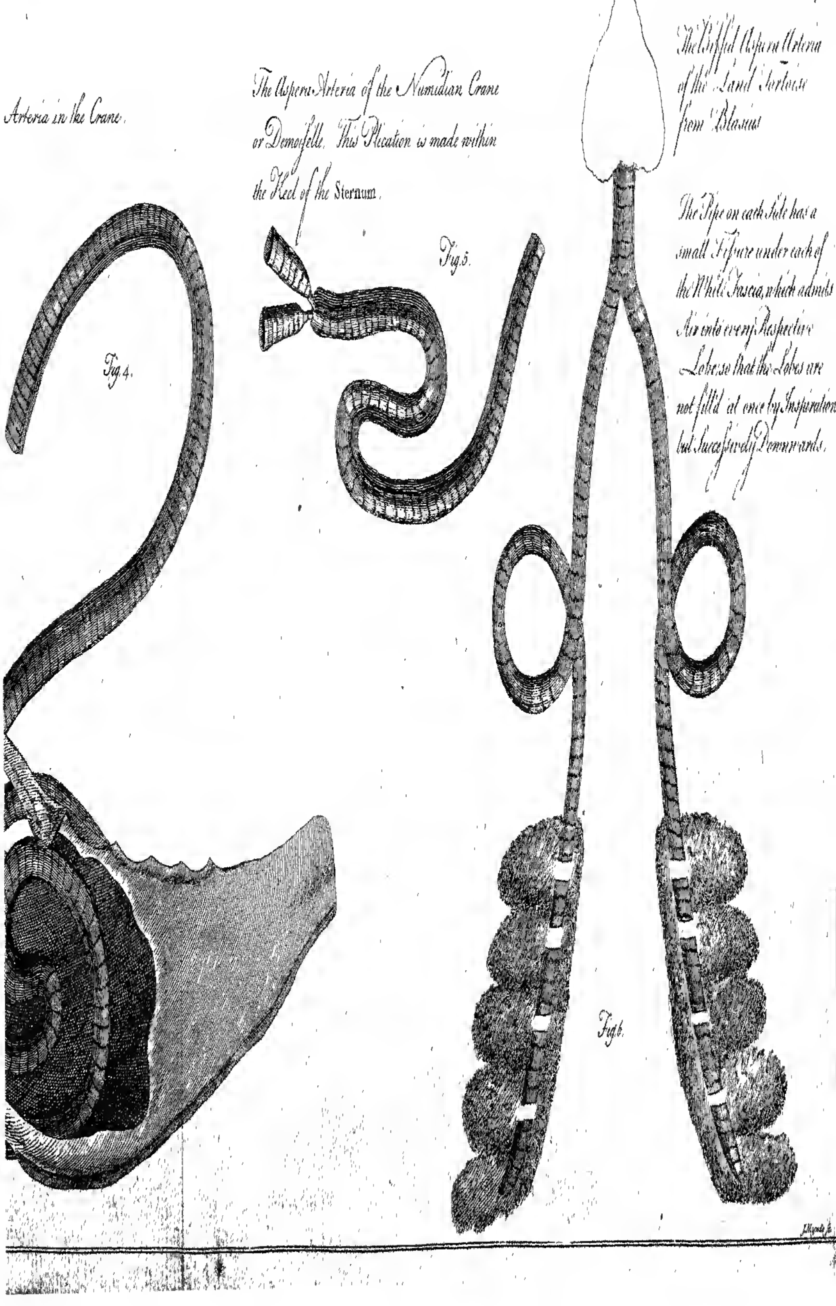

Arteria in the Crane.
Fig. 4.
The Aspera Arteria of the Numidian Crane or Demoiselle. This Plication is made within the Keel of the Sternum.
Fig. 5.
The Bifid Aspera Arteria of the Land Tortoise from Blasius
The Pipe on each Side has a small Fissure under each of the White Fascia, which admits Air into every Respective Lobe; so that the Lobes are not fill'd at once by Inspiration, but Successively Downwards.
Fig. 6.

nution of the bulk of the tortoiſe; and it is probable that when the tortoiſe endeavoured to ſink to the bottom, the water fell in the pipe, becauſe the animal leſſened its bulk by the contraction of its muſcles; and that the water roſe by the ſlackening of the muſcles, which, ceaſing to compreſs the lungs, permitted it to return to its firſt ſize, and rendered the whole body of the tortoiſe lighter. I have, in many kinds of fiſh, diſſected their ſwimming bladders, and found that in great and ſmall theſe are veſted with a ſtrong muſcular membrane, which they are capable of contracting and dilating at will, whereby they are able to compreſs or expand the column of air within very conſiderably; this bears ſome analogy to the detruſor muſcle of the human urinary bladder, in contracting itſelf for the expulſion of the urine.

Tab. X. Fig. 1. Repreſents the ſituation and incloſure of the aſpera arteria of the wild ſwan, in a lateral view.

2. A view of the ſame, with its progreſs within the thorax upon the ſternum.

3. The aſpera arteria of the Indian cock.

Tab. XI. Fig. 4. A lateral view of the aſpera arteria of a crane, as ſituated in the cavity formed in the ſternum.

5. Shews the flection of the aſpera arteria of the Numidian crane.

6. The biffid aſpera arteria of the land tortoiſe.

Received

Received June 6, 1766.

XXV. *A Letter from Mr.* William Mountayne, *F. R. S. to the Right Honourable* James *Earl of* Morton, *President of the Royal Society, containing some Observations on the Variation of the Magnetic Needle, made on Board the* Montagu *Man of War, in the Years* 1760, 1761, *and* 1762, *by Mr.* David Ross, *Surgeon.*

My Lord,

Read June 13, 1766.

THE following tables I have compared with the Variation Chart, published in the year 1756, and do find that they agree pretty well in general, making allowance for the time elapsed *: it is true, that, in some few places in the Atlantic Ocean, they differ; yet this may probably arise, as is often the case, from an error in the Montagu's supposed longitude, where such observations were made. But the greatest difference (a greater than should arise, I think, according to common course) appears upon the coast of Portugal, Cape Saint Vincent, and about Gibraltar, near and within

* By this expression, I do not mean that the variation undergoes a regular and uniform alteration, notwithstanding such appearance, but rather suspect the contrary.

sight

ſight of land, where the obſervations are aſcertained to the ſpot. Hence, if mine obſerved about the year 1756, and thoſe of Mr. Roſs's, were both near the truth, at the reſpective times when they were taken, I know not how to account for this conſiderable encreaſe, unleſs thoſe late extraordinary convulſions, in the bowels of the earth, upon thoſe ſeveral coaſts, may be found, by further experiments, to have there influenced the directions of the magnetic needle.

When theſe obſervations firſt came to my hands, I intended to employ them, among others, in the conſtruction of a variation chart, at ſome proper period of time; but, by reaſon of a long and ſevere indiſpoſition, I am obliged to ſuſpend thoſe my intentions. As, therefore, they appear to be made by a judicious and accurate obſerver, and, as Mr. Roſs remarks, upon ſome local attractions in the Weſt-Indies, mentioned at the cloſe of his letter (which, without doubt, obtains in many other places) may induce ſome able philoſopher, ſkilled in theſe matters, to inveſtigate the cauſe of thoſe phænomena; I beg leave to addreſs the whole to your lordſhip, to be diſpoſed of, as you ſhall think proper. I am,

My Lord,

Your Lordſhip's moſt obedient ſervant,

W. Mountaine.

Gainsford-ſtreet, Southwark, May 18, 1766.

Extract of a Letter from Mr. David Roſs, *Surgeon of His Majeſty's Ship the* Montagu, *to Mr.* William Mountaine, *F. R. S. relating to the Variation of the Magnetic Needle; with a Sett of Obſervations made by himſelf, on Board the ſaid Ship, during the Years* 1760, 1761, *and* 1762.

N° 6, in Virginia-Street, April 9, 1763.

SIR,

REPEATED obſervations of the variation of the magnetic needle, when the ſpot they are taken upon can be tolerably well aſcertained, are of great ſervice to philoſophy in general, but particularly to navigation, as in future ages they may ſerve as a baſis, to found its theory upon.

Having had an opportunity of obſerving, with all the care and accuracy in my power, the deflection of the magnetic needle, by an azimuth compaſs of Dr. Gowen Knight's conſtruction, during a paſſage to the Weſt-India Iſlands, a cruiſe there, and return to Britain; and likewiſe a cruiſe about the Straits of Gibraltar, I with pleaſure communicate them to you, as a perſon deſerving the higheſt eſteem of the commercial part of Britain, by your excellent new edition of the Variation Chart of the late celebrated Dr. Halley; and I beg, Sir, your favourable acceptance of them.

I can aſſure you, I have endeavoured, as far as the thing would admit of, to get at the truth, having, at every obſervation, not truſted to a ſingle azimuth, but

but generally have taken a medium of, from ſix to ten, or more.

I cannot help taking notice of what ſurprized me greatly, viz. that I ſhould always find the variation leſs at anchor, in the Weſt-India Iſlands, than at ſea, though near the ſame ſpot. As for inſtance, June 18, 1760, three leagues ſouth Prince Rupert's Bay, Dominico, I found the variation, by a medium of five azimuths and an amplitude, to be $5° 27'$ E. And in the Bay itſelf, June 20, by three azimuths and an amplitude, it was $3° 27'$; and on the 22d, by ſix azimuths and amplitude $= 3° 12'$ E. The medium of theſe is $3° 20'$ E. full $2° 7'$ leſs than in the offing, though but nine miles off.

The ſame phænomenon appeared, upon experiments repeated March the 23d, 1761, when in the ſame Bay, the medium of eight azimuths gave $3° 19'$ E. Off of Antigua, and at anchor in Saint John's Road, the ſame thing was obſerved, though the difference was not ſo great. Theſe things exerciſed my thoughts, though I muſt own I could not hit on any ſolution that pleaſed myſelf. I am,

SIR,

with great truth,

your very humble ſervant,

David Roſs.

A TABLE of the Variations of the Magnetic Needle, as obſerved on Board His Majeſty's Ship Montagu, in the Years 1760, 1761, and 1762, by Mr. David Roſs; and communicated by Mr. William Mountaine, F.R.S.

Time when.			Latitude			Long. fr. Lon.			Variation.		
1760	April	12	Off Bell-Iſle and Grois.						19°	11′	W.
		26	43°	23′	N.	11°	15′	W.	18	51	W.
		29	40	20	N.	13	39	W.	19	11	W.
	May	2	40	18	N.	18	36	W.	17	0	W.
		5	34	44	N.	21	42	W.	14	30	W.
		6	31	38	N.	22	25	W.	14	15	W.
		7	30	7	N.	23	12	W.	13	50	W.
		9	28	51	N.	25	39	W.	10	4	W.
		10	27	2	N.	28	40	W.	10	30	W.
		11	26	10	N.	30	30	W.	10	16	W.
		12	25	9	N.	32	37	W.	7	10	W.
		13	24	3	N.	34	14	W.	5	59	W.
		14	22	51	N.	35	51	W.	5	0	W.
		15	21	40	N.	37	33	W.	4	20	W.
		17	19	8	N.	41	27	W.	1	30	W.
		18	18	4	N.	43	18	W.	1	0	W.
		19	16	58	N.	45	9	W.	0	20	W.
		20	16	7	N.	46	50	W.	0	30	E.
		21	14	52	N.	48	39	W.	1	7	E.
		22	13	38	N.	50	28	W.	1	59	E.
		23	13	13	N.	51	48	W.	2	12	E.
		25	13	30	N.	56	5	W.	3	45	E.
		26	13	10	N.	57	50	W.	3	58	E.

Time when.		Variat. ° ′
1760 May 28, 29, and 31	In Carlisle, Barbadoes	4 3E.
June 1	Barbadoes S. E. 10 ½ leagues dist.	5 20E.
2	15° 19′ N. 59° 0′ W.	5 12E.
3	16 16 N. 59 18 W.	4 24E.
4	Antigua W. N. W. dist. 6 leagues	4 51E.
6	Redondo E. ½ N. 4 leagues.	4 50E.
8	Montserratt S. by E. Redondo S. by W.	5 32E.
15	In the pas. bet. Martinique and Dominico	5 6E.
16	Off the S. W. end of Martinique	5 41E.
17	Off the S. end of Dominico	5 21E.
18	S. of Rupert's Bay, Dominico, dist. 3 leag.	5 27E.
20, 22	In Prince Rupert's Bay — 3° 27′ Dominico —— 3 12	3 20E.
July 11	Deseada S. W. by W. ½ W. dist. 3 leag.	5 3E.
12	15° 0′ N. 59° 7′ W.	5 35E.
September 15, 16, 18	In Grand Courland's Bay At the South West end of Tobago, the medium is	4 32E.
October 5	N. end of Tobago E. N. E. ½ E. dist. 5 leag.	4 45 E.
6	D° E. by S. dist. 4 leagues	5 0E.
10	Island Bequia N. W. by N. ½ W. dist. 7 leag.	4 0E.
14	Off the N. W. end of Saint Lucia	5 14E.
15	Off Saint Pierre's Martinico	5 17E.
22	At the wind of the Channel between Martinique and Dominico	5 19E.
23	At the East end of the same	5 36E.
24	Saint Lucia W. ¼ N. dist. 15. leagues	5 34E.
1761 March 23	In Prince Rupert's Bay, Dominico	3 19E.
April 4	In St. John's Road, Antigua	3 9E.
15	Off the east end of Antigua	4 31E.
May 4	In Carlisle Bay, Barbadoes	3 47E.
July 27	At the Dog and Prickly Pear	4 44E.

Time when.			Latitude in			Long. fr. Lon.			Variation.		
			°	′		°	′		°	′	
1761	July	28	19	35	N.	62	50	W.	4	35	E.
		29	20	46	N.	62	43	W.	4	27	E.
		30	22	20	N.	62	30	W.	2	42	E.
		31	23	42	N.	62	36	W.	2	15	E.
	Auguſt	1	25	5	N.	62	37	W.	1	38	E.
		2	26	20	N.	62	57	W.	1	18	E.
		3	27	26	N.	62	37	W.	0	44	E.
		4	27	56	N.	62	43	W.	0	3	E.
		5	28	5	N.	62	50	W.	0	23	E.
		6	28	11	N.	62	35	W.	0	1	E.
		7	28	40	N.	62	59	W.	0	27	W.
		8	29	23	N.	63	1	W.	0	48	W.
		9	30	1	N.	62	59	W.	1	33	W.
		10	31	1	N.	62	21	W.	1	58	W.
		11	32	41	N.	60	37	W.	2	45	W.
		12	33	58	N.	59	14	W.	4	13	W.
		13	35	3	N.	57	54	W.	5	0	W.
		15	36	31	N.	54	57	W.	7	4	W.
		16	37	33	N.	52	55	W.	8	20	W.
		17	37	59	N.	51	4	W.	9	40	W.
		18	38	8	N.	50	13	W.	10	20	W.
		19	38	36	N.	48	41	W.	10	12	W.
		20	38	23	N.	47	48	W.	10	57	W.
		21	38	57	N.	46	30	W.	11	52	W.
		22	39	22	N.	45	52	W.	12	0	W.
		23	40	13	N.	44	29	W.	12	0	W.
		24	41	8	N.	42	26	W.	12	0	W.
		26	41	31	N.	41	42	W.	13	0	W.
		27	42	0	N.	39	58	W.	13	35	W.
		28	42	38	N.	38	11	W.	13	36	W.
		29	43	15	N.	36	21	W.	14	0	W.
		30	43	0	N.	35	53	W.	14	30	W.
		31	43	28	N.	34	40	W.	14	30	W.
	September	1	44	45	N.	31	58	W.	17	11	W.
		2	45	36	N.	29	49	W.	18	51	W.
		4	48	14	N.	25	17	W.	18	31	W.
		5	48	57	N.	23	13	W.	19	37	W.
		6	49	11	N.	20	28	W.	19	30	W.
		7	50	35	N.	18	17	W.	19	52	W.

Time when.		Variation.
1761 Sept. 10	Off Cape Clear in Ireland	19 55 W.
20	Scilly S. S. E. dist. 5 leagues	19 55 W.
21	The Start E. ½ N. dist. 16 miles	20 16 W.
December 8	36° 25′ N. 7° 45′ W.	16 53 W.
19, 25, and 26	In Gibraltar Bay	17 11 W.
1762 Jan. 23	35° 46′ N. 7° 2′ W.	18 3 W.
Jan. and Feb.	Between Cape Saint Vincent and the Straits Mouth	17 31 W.
February 11	In Cascais Road	17 32 W.
D° 12, 23, and March 2	In Lisbon River	17 32 W.
5	37° 56′ N. 9° 55′ W.	17 29 W.
	Betwen Cape Saint Mary's and Cape Spartel, the medium is	17 15 W.
	From 3 to 12 leag. E. off Gibral. Hill on the European Shore	17 20 W.
	From 3 to 12 leag. E. off Gibral. Hill on the Barbary Shore	18 30 W.

Received.

Time when.			Latitude in			Long. fr. Lon.			Variation.		
			°	'		°	'		°	'	
1761	July	28	19	35	N.	62	50	W.	4	35	E.
		29	20	46	N.	62	43	W.	4	27	E.
		30	22	20	N.	62	30	W.	2	42	E.
		31	23	42	N.	62	36	W.	2	15	E.
	August	1	25	5	N.	62	37	W.		38	E.
		2	26	20	N.	62	57	W.	1	18	E.
		3	27	26	N.	62	37	W.	0	44	E.
		4	27	56	N.	62	43	W.	0	3	E.
		5	28	5	N.	62	50	W.	0	23	E.
		6	28	11	N.	62	35	W.	0	1	E.
		7	28	40	N.	62	59	W.	0	27	W.
		8	29	23	N.	63	1	W.	0	48	W.
		9	30	1	N.	62	59	W.	1	33	W.
		10	31	1	N.	62	21	W.	1	58	W.
		11	32	41	N.	60	37	W.	2	45	W.
		12	33	58	N.	59	14	W.	4	13	W.
		13	35	3	N.	57	54	W.	5	0	W.
		15	36	31	N.	54	57	W.	7	4	W.
		16	37	33	N.	52	55	W.	8	20	W.
		17	37	59	N.	51	4	W.	9	40	W.
		18	38	8	N.	50	13	W.	10	20	W.
		19	38	36	N.	48	41	W.	10	12	W.
		20	38	23	N.	47	48	W.	10	57	W.
		21	38	57	N.	46	30	W.	11	52	W.
		22	39	22	N.	45	52	W.	12	0	W.
		23	40	13	N.	44	29	W.	12	0	W.
		24	41	8	N.	42	26	W.	12	0	W.
		26	41	31	N.	41	42	W.	13	0	W.
		27	42	0	N.	39	58	W.	13	35	W.
		28	42	38	N.	38	11	W.	13	36	W.
		29	43	15	N.	36	21	W.	14	0	W.
		30	43	0	N.	35	53	W.	14	30	W.
		31	43	28	N.	34	40	W.	14	30	W.
	September	1	44	45	N.	31	58	W.	17	11	W.
		2	45	36	N.	29	49	W.	18	51	W.
		4	48	14	N.	25	17	W.	18	31	W.
		5	48	57	N.	23	13	W.	19	37	W.
		6	49	11	N.	20	28	W.	19	30	W.
		7	50	35	N.	18	17	W.	19	52	W.

Time when.		Variation.
1761 Sept. 10	Off Cape Clear in Ireland	19 55 W.
20	Scilly S. S. E. dift. 5 leagues	19 55 W.
21	The Start E. ½ N. dift. 16 miles	20 16 W.
December 8	36° 25′ N. 7° 45′ W.	16 53 W.
19, 25, and 26	In Gibraltar Bay	17 11 W.
1762 Jan. 23	35° 46′ N. 7° 2′ W.	18 3 W.
Jan. and Feb.	Between Cape Saint Vincent and the Straits Mouth	17 31 W.
February 11	In Cafcais Road	17 32 W.
D° 12, 23, and March 2	In Lifbon River	17 32 W.
5	37° 56′ N. 9° 55′ W.	17 29 W.
	Betwen Cape Saint Mary's and Cape Spartel, the medium is	17 15 W.
From 3 to 12 leag. E. off Gibral. Hill on the	European Shore	17 20 W.
	Barbary Shore	18 30 W.

Received.

Received June 5, 1766.

XXVI. *A Letter to the Prefident of the Royal Society, containing a new Manner of meafuring the Velocity of Wind, and an Experiment to afcertain to what Quantity of Water a Fall of Snow is equal.*

Kirknewton, May 13, 1766.

My Lord,

Read June 19, 1766.

I Should think myfelf moft unworthy of the honour which your lordfhip and the Royal Society have done me, if the notice which you was pleafed to take of my letter upon the late comet, did not make me more careful to obferve whatever, I thought, might tend to improve the knowledge of nature, which is a capital part of the laudable defign of the Society.

Your lordfhip knows, that my fituation expofes me to every blaft that blows, and affords a fair opportunity for meafuring the velocity of the wind (the force of which I am, fo often, obliged to feel). I have attempted to determine this by letting light downy feathers fly in the wind (the method, I underftand, ufed by the ingenious Dr. Derham); but cannot fay, in all the trials I have made (though I have let fifty of thefe feathers fly, one after the other, at a time), that I have ever feen above one, or two at moft, upon which I could have founded a calculation. The velocity of the wind near the earth is very unequal, upon account of the

the frequent interruptions it meets with from hills, trees, and houſes; and even in open plains, the ſurface of the earth, though much ſmoother than it commonly is, muſt reflect, and interrupt ſuch a fluid as the air, and occaſion great irregularity in the velocity of its current: this is the reaſon, when a feather is let fly with the wind, why it ſeldom, if ever, deſcribes a ſtrait line, but moves ſometimes in a kind of ſpiral, now high, and then low, ſometimes to the right, and then again to the left; and why two feathers let fly at once, ſeldom, if ever, keep together, or deſcribe ſimilar lines.

But, at ſome conſiderable diſtance from the earth, the velocity of the wind ſeems to be regular and ſteady: nothing can be more uniform, than the velocity of a cloud in the ſky appears to be, even in the greateſt ſtorm: it is like a ſhip carried away inſenſibly by a ſmooth and gentle current, paſſing over equal ſpaces in equal times. This ſuggeſted the thought, that the motion of a cloud, or its ſhadow, over the ſurface of the earth, would be a much more proper meaſure of the velocity of the wind.

In the end of March 1763, I had as favourable an opportunity of putting this method into practice, as I could have wiſhed for; the ſtorm was exceeding high, and moved with vaſt velocity; the ſun was bright, the ſky clear, except where it was ſpotted with light floating clouds; I took my ſtation in the north window of my dining room, near the clock, from which I had a free proſpect of the fields; the ſun was in the meridian, the wind due weſt interſecting his rays at right angles; I waited until the fore-part of the ſhadow of a cloud, that was diſtinct, and well

defined, juſt touched a ſouth and north line, which I had marked upon the ground; at that inſtant I began my reckoning, and followed the ſhadow with my eye in its progreſs, counting ſeconds all the while by the clock, until I had reckoned up 15 ſeconds; then I obſerved exactly where the foreſaid edge of the ſhadow was. This experiment I repeated ten times in half an hour, and ſeldom found the difference of a ſecond, in the time which different clouds took to move over the ſame ſpace. On the 5th of May current, I repeated the trial four different times, the ſun being alſo near the meridian, the wind in the weſt, with light clouds floating in a clear ſky as formerly; and found that the ſhadows of different clouds took ſome of them 44, and others 45 ſeconds, to paſs over the ſame ſpace which they had moved over in 15 ſeconds, in the former trials.

	Feet.	
This ſpace meaſures exactly	1384 =	ſpace paſſed over in 15 ſeconds,
which multiplied by	4	
gives	5536 =	ſpace paſſed over in one minute,
which multiplied by	60	
gives	332,160 =	ſpace paſſed over in one hour.

Which ſpace is = 62.9 Engliſh miles per hour, the velocity of the wind in March 1763.

One third of this (or 21 miles nearly) ſhews the velocity of the wind on May the 6th, when it blew a freſh gale.

This day, May 12, there was a ſmall weſterly breeze, the velocity of which I meaſured upon the ſame line, the ſun being 10 minutes paſt the meridian, and found that the ſhadow took 95 ſeconds to paſs over the above

above ſpace, which gives the velocity of the wind at the rate of 9.9 Engliſh miles per hour.

Thus, by having ſeveral lines in different directions of a known length marked upon the ground, one may eaſily (and with great accuracy, I imagine), meaſure the velocity of the wind. If a perſon was provided with an inſtrument for meaſuring the force of the wind, it would perhaps be worth while to obſerve, whether, when the velocities of different winds were the ſame, (or nearly ſo) the forces of theſe winds did not vary with the ſeaſons of the year, the points of the compaſs from which the wind blows, and alſo with the different ſtate of the barometer and thermometer, ſince the momentum of the wind depends not only upon its velocity, but alſo upon its denſity.

From the end of March 1765, to the end of March laſt, we, in this part of Scotland, had very little rain, and leſs ſnow in proportion; our rivers were as low, through the winter, as they uſe to be in the middle of ſummer; ſprings failed in moſt places, and brewers and maltſters were obliged, even in winter, to carry their water at a conſiderable diſtance; I was much afraid there would not be moiſture enough in the earth for the purpoſes of vegetation, if this ſeaſon ſhould ſet in as dry as the former, before we got a new ſupply of rain. In the end of March laſt, we had a fall of ſnow; and, as I did not remember to have ever read an account of ſuch an experiment, I wiſhed to be able to determine, to what quantity of rain this fall of ſnow was equal.

The ſnow had been falling from five o'clock the former evening, till ten o'clock next day; about eleven o'clock I meaſured the depth of the ſnow, and found

 it

it to be 6.2 inches; then I took a ſtone jug, holding about three Engliſh pints, and turned the mouth of it downwards upon the ſnow meaſured, and where the ground below was ſmooth, and hard; and by this means I took up all the ſnow from top to bottom in the jug; this ſnow I melted by the ſide of a fire, and the 6.2 inches of ſnow yielded ſix tenths of an inch deep of water in the ſame jug. After emptying the jug, I dried, and weighed it in a balance, and took up the ſame quantity of ſnow in it as before, weighed it again, and found the weight of the ſnow taken up, and from this weight computed what quantity of water it ſhould have produced, and found that it ought to have produced ſix tenths of an inch and $\frac{1}{20}$ of an inch more: then I diſſolved the ſnow, and found that it yielded a quantity of water in the bottom of the jug, ſix tenths of an inch deep as in the former experiment. The difference of $\frac{1}{20}$ of an inch in the depth of the water, betwixt the weight and the melting of the ſnow, was probably owing to an exhalation from the jug, while the ſnow was melting by the fire, for I obſerved a ſteam ſometimes riſing from it. A greater or leſſer degree of cold, or of wind, while the ſnow falls, and its lying a longer or ſhorter while upon the ground, will occaſion a difference in the weight and in the quantity of water produced from a certain number of cubic feet, or inches, of ſnow; but, if I may truſt to the above trials, (which I endeavoured to perform with care) ſnow, newly fallen, with a moderate gale of wind, freezing cold, which was the caſe of the ſnow I made the trials upon, the 27th of March laſt, will produce a quantity of water equal to $\frac{1}{10}$ part of its bulk; or the earth, when covered with ſnow, ten

ten inches deep, will be moiſtned by it when melted; rivers, and ſprings recruited, as much as if a quantity of rain had fallen that covered the ſurface of the earth to the depth of one inch.

I am, my Lord,

with the greateſt reſpect,

your Lordſhip's moſt obedient

and moſt devoted humble ſervant,

Alex. Brice.

Received June 5, 1766.

XXVII. *Some Obſervations on the Country and Mines of* Spain *and* Germany, *with an Account of the Formation of the Emery Stone; from* William Bowles, *Eſq; Director General of the Mines of* Spain; *communicated by* P. Collinſon, *F. R. S.*

Read June 19, 1766.

AT the extremity of Old Caſtile, in Spain, is ſituated a territory called Montana, which is divided into two parts; the Low Montana is that chain of mountains, which bounds the

the Cantabrian Sea. The city Santander is its chief port, from whence you afcend foutherly, twelve long leagues, a fucceffion of high craggy mountains, to the town of Reynofa in the upper Montana, which extent ftretches three leagues more, and then you continually defcend about fourteen leagues to the city of Burgos, the capital of Old Caftile.

Reynofa is in the center of an open plain, furrounded by a ridge of high mountains, at whofe feet are low hills of pafture-land.

To the weft of Reynofa, in an hour's walk, is the fource of the great river Ebro, which receives all the waters on that fide, and conveys them into the Mediterranean, feven leagues below the city Tortofa.

All the fpring, rain and fnow waters, of the mountains to the north of Reynofa, run into the Bay of Bifcay.

The waters, from the fouth chain of the mountains, are collected in the river Pifverga, which runs into the river Duero, and from thence are carried to the Atlantic ocean at Oporto.

Hence we fee, that the adjacent parts of Reynofa divide the waters of the three feas, which lye north, eaft, and weft.

Eight leagues fquare of this upper Montana is the higheft land in Spain; the mountains rife to the atmofphere to the line of congelation; I fee fnow from my window this 4th of Auguft, as writing this letter. Some years ago there ufed to fall fo much fnow, that the people were forced to dig lanes through it, to go to church, in the winter; but there has fallen little fnow fince the earthquake at Lifbon, and fome years none at all. I am perfuaded, it changed the climates

of many parts of Spain; for no man living ſaw, nor heard his father ſay he ſaw, ſnow fall in or about the city of Sevil, until the year 1756.

I found many plants in theſe mountains, which I remember to have ſeen in Switzerland; they abound with oak, beech, birch, holly, aud hazel.

The hills and plains are fine paſture; I never ſaw a meadow in any other part of Spain, neither did I ſee horſes and cows feed on hay any where elſe.

Theſe mountains are formed of ſand-ſtone, lime-ſtone, plaſter-ſtone (or gypſum) and emery-ſtone.

The ſand-ſtone, is at the ſummit of the mountains, and ſome hills, and the lime-ſtone forms the body; but the contrary is ſeen in others, the ſand-ſtone abounds, and the plaſter is always loweſt.

As for example, the high mountain of Arandilla, which is a ſmall league off the town, is all ſand-ſtone at the ſummit; its body is a maſs of aſh-coloured lime-ſtone, in which is found impriſoned petrified cornu ammonis, and ſcollop ſhells; and there are beds of plaſter-ſtone at its foot, towards the plain; theſe join to a ſtratum of black marble veined white and yellow, which is no more than a purer kind of lime-ſtone, as all other marbles are.

On the hill to the eaſt of Reynoſa, and in the plain, are found great blocks of emery-ſtone, of which I will ſay a word, becauſe I think its nature is not truely known; at leaſt that of Spaniſh emery, which the looking-glaſs grinders of the king's fabric at St. Ildefonſo ſay is the moſt biting emery, they ever uſed; and I never ſaw any other in its native matrix..

That

That iron has been, and is now, in a fluid ſtate, percolating through the earth, and that it ſubſides, chryſtaliſes, or is precipitated, to form different bodies, is demonſtrated by the black and red bloodſtone, by ſome beautiful ſtalactites, which are almoſt pure iron, by the eagle-ſtone, by figured pyrites, by native vitriol, and by native crocus.

When this fluid iron penetrates a rock of ſand-ſtone, and only ſtains the ſurface of each grain, of a browniſh, reddiſh, or yellow colour, it becomes only ſand and crocus; but, when it is joined with the chryſtaline matter in a fluid ſtate, in the very act of chryſtaliſation of each grain of ſand it incorporates with it, its weight and hardneſs is increaſed, and then it becomes emery.

The earth of the mountains and hills is of the ſame nature as that of the rock below. If it is lime-ſtone, the ſoil caſt into any acid liquor will boil up with a violent efferveſcence, and the acid will diſſolve it.

If the rock below be ſand-ſtone, or plaſter-ſtone, or emery, the earth of that hill or mountain will remain quiet in the acid, and there is no efferveſcence nor diſſolution.

I have often obſerved, that, when the rocks below are mixed, calcary and noncalcary, the ſoil of the ſurface is alſo of a mixt nature; and I always found the action of the acid to be weak or ſtrong upon theſe earths, in proportion to the ſort of ſtone with which they abound.

Thirty one leagues ſouth eaſt of Madrid, and five leagues ſouth of the ſource of the river Tagus, is ſituated the town of Molina Aragon, capital of a

lordſhip of the crown, almoſt in the center of Spain; the high hills of this little territory are covered with pine trees; here I learned ſome truths, which prove, that the following opinions ought to be ranked amongſt vulgar errors.

Firſt, that ſalt-ſprings are not found in the high primitive mountains, but in the low hills and plains only.

The elevated town of Molina, and the rocky country about it, is formed of red and grey ſand-ſtone, lime-ſtone, white and grey granite. Theſe rocks contain either ſalt, or ſalt-petre; the houſes built of this ſtone are covered with the ſaline effloreſcences, which are drawn out by the ſun after rain. The whole territory of Molina is full of ſalt ſprings; but there is a copious ſalt-ſpring, riſing out of a land yet higher than the ſource of the Tagus, and not far from it, which is one of the higheſt territories in all the inland parts of Spain, for it divides the waters of the ocean and Mediterranean. This ſpring furniſhes ſalt to the juriſdiction and biſhoprick of Albarrazen. There is beſides another ſalt ſpring, in another elevated ground, which ſupplies the eighty two towns and villages of Molina-Aragon with ſalt: beſides which, there is a ſalt ſpring, iſſuing out of a ſpot in the Montana, which is higher than the fountain of the Ebro, and about a quarter of a mile diſtant from it.

Secondly, that metallic vapours deſtroy vegetation; the following inſtances evince the contrary.

There are many iron, copper, lead, and pure pyritous ores, in theſe mountains; and yet the ſame plants, and the ſame ſweet graſs grow there as in other parts, of which I will give a more particular account.

About two hours walk northweſt of Molina, there is a little hill called Platilla; it extends about half a league over, from valley to valley; its body is a ſolid, rocky, white granite, through which run, in different directions, and without any order, an infinite number of blue, green, and yellow veins of rich copper oar, which holds a little ſilver, mineralized by a great quantity of arſenick and ſulphur: the very ſurface of the rock is in many places ſtained blue, and green, and the veins of oar are not above a foot deep. In the fiſſures, and in the ſolid rock, is contained lead oar, which is ſometimes found even on the ſurface; and yet the following plants grow out of the ſoil, which covers theſe arſenical ſulphurous veins, and is not more than a foot deep; true oak, flax, white thorn, juniper, cyſtus, wild-roſe, uva urſi, phlomis, verbaſcum, ſtœchas, ſage, thyme, ſerpillum, roſemary, and many others, which it would take up too much time to mention. The earth of this ſame hill is covered with the ſame ſweet ſmall graſs as the reſt of the country.

I have alſo made the ſame obſervations, out of Spain, at the three greateſt mines in Europe, viz. St. Mary of the mines in Alſatia; Clauſthal, in the Hartz-mountains of Hanover; and Frayberg, in Saxony.

The mines of St. Mary are at the head of a valley. Its hills are ſome of them covered with oak, pines, and others with apple, pear, plum and cherry, and others, with fine graſs downs. The tops of others are fields of wheat, which, in the year 1759, as I found by my notes, gave a produce of eight for one. All theſe vegetables grow in a ſoil, a foot or two deep, which

which covers a rock, full of the moſt arſenical, ſulphureous, ſilver, copper, lead, and cobalt ores, in Europe, and moſt of the veins are near the ſurface.

The mines of Frayberg are in low hills near the city. I ſaw them all covered with barley in July. A ſtranger would not imagine that men were reaping corn over hundreds of miners heads, who were blowing up veins of ore, arſenick, and brimſtone.

The mines of Clauſthal are in a plain, which, in truth, is the ſummit of a mountain; the Dorothy and Caroline veins of ſilver, lead, and copper ore, ſtretch away eight miles to the Wild-Man mountain; the fineſt meadows and ſweeteſt graſs are upon theſe veins, and all their branches near the city: they feed nine hundred cows, and two hundred horſes; they are mowed in June, and a ſecond crop ſprings up, which is mowed in Auguſt: a multitude of plants grow in theſe high meadows, over the mines.

It is true, I ſaw mines in the barren naked mountains and hills: but it is certain that their barrenneſs is not the effect of mineral vapours; but the air, moiſture, heat, and cold, have more power over the ſurfaces of ſome rocks, than of others, to moulder the ſtone into earth. Such is the high mountain of Ramelſberg, above Goſlar, whoſe inhabitants have lived by the mines found therein. I crept up this ſteep rock to its ſummit; I found it ſplit and cracked into millions of fiſſures, from one foot to an inch wide; in other places, it was ſhivered into ſmall rotten ſtones, which became a receptacle for a few plants, graſs, moſs, &c. and, as this decayed ſtone moulders into earth, it will be more abundant in vegetable productions; this may, perhaps, have been the origi-

nal ſtate of thoſe mountains, which are now covered with verdure.

Communicated by

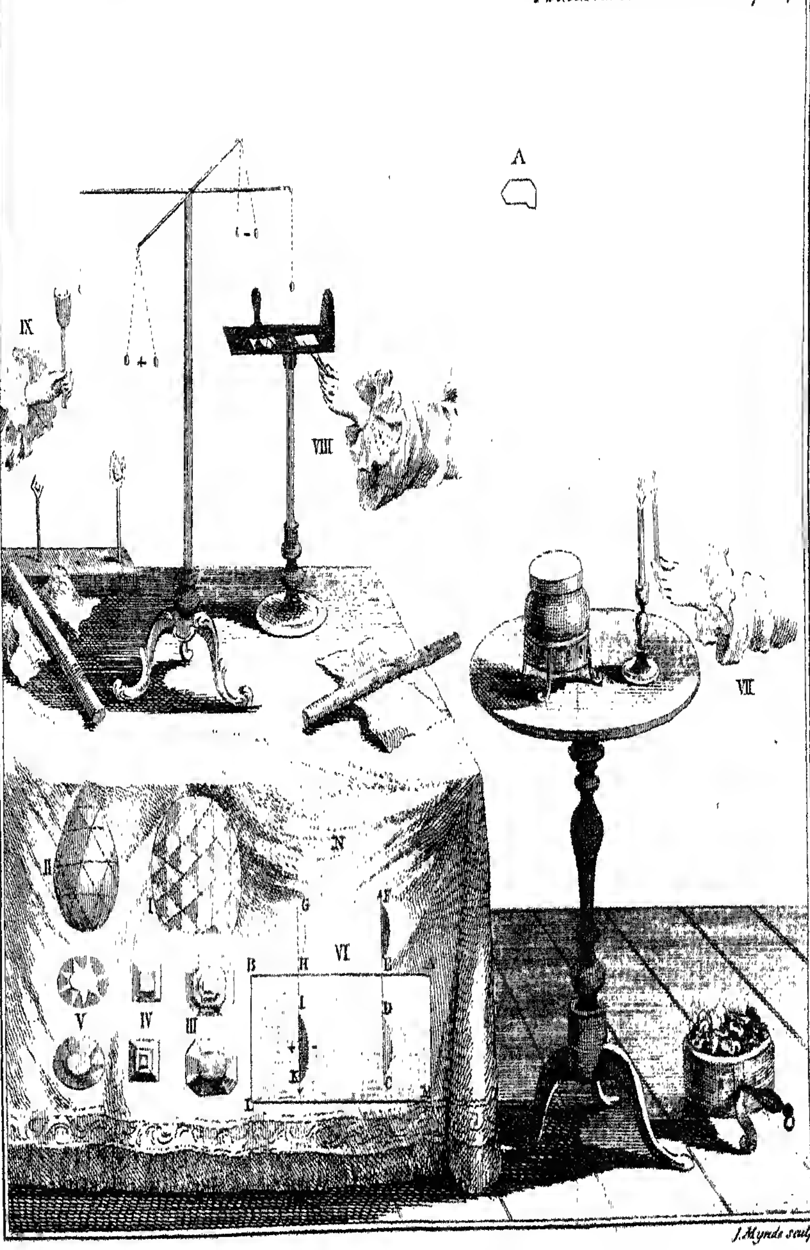
Madrid, Jan. 1765.

P. Collinſon.

XXVIII. *Commentarius de indole Electrica Turmalini, auctore* Torberno Bergman, *Mathematatum & Philoſophiæ Naturalis ad Reg. Academiam* Upſ. *adjuncto, Academiæ Imper. N.C. Academiæ Reg. Scientiarum* Stockholmenſis *& Societatis Regiæ* Londinenſis *membro.*

Read Nov. 20, 1766.

I. CUM circa finem anni præteriti quinque ſelecti turmalini, ab Academia Regia Scientiarum Stockholmenſi nuper adquiſiti, in meas inciderent manus, venia illuſtris hujus Societatis varia hiſce inſtitui tentamina, ut ſingularium horum lapillorum naturam, ſi fieri poſſet, penitius expiſcarer. Fateri vero convenit, me parum ſucceſſus ſperaſſe, poſtquam celeberrimi noſtri ævi phyſici, Æpinum loquor & Wilſonum, turmalinum adtentione, viris hujus ordinis propria, ſcrutati fuerant. Et ſane, ſi res eſſet dijudicanda ex numero ſcriptorum admodum parvo;



J. Mynde sculp.

parvo, etsi indoles eorum electrica dudum ante novem innotuerit annos, in eam facile incidissem opinionem, tales lapides ex Indiis minori molestia comparari, quam novi quid iisdem inveniri. Ast dissensus memoratorum physicorum circa nonnulla experimenta fecit, ut non omnem abjicerem spem.

Omnia instituta tentamina enumerare tanto minus necessarium duco, cum non tantum hæc singula, sed etiam reliqua antea nota, ex unica simpliceque dependeant lege generali. Antequam vero hanc explico, ipsi turmalini breviter sunt describendi. Præterea, quidquid Domini Æpinus & Wilson de eorum proprietatibus scripserint, in sequentibus pro notis pono, ne longo anfractu circumscribatur disquisitio.

II. Primus & maximus coloris est fere nigri, tenues vero pellucidique margines rufescunt. Alterum latus est planum, alterum convexum arteque in triangula divisum, uti TAB. XII. Fig. 1. monstrat. In utroque ex modo descriptis lateribus unus est polus. Sub itinere margo paulum distractus fuit, & granum quartsi, adhuc in fractura remanens, fragilitatis caussam indicat. Damnum hinc magnitudini illatum habitus solertissimi mineralogi, Domini Rinman, resarcivit, quippe qui particulis divulsis talia instituit experimenta, ut in regno minerali mirando huic lapidi conveniens adsignari queat locus, quod, quantum scio, nemo prius tentavit. De hoc vero plura adferre, a præsenti scopo foret alienum.

Secundus paullum minor, fusco flavescens, ab utroque latere æqualis & similis figuræ 2. Poli sunt oppositi in margine laterali, radiique lucis, ab altero in alterum, transire nequeunt lapillum in omni alia directione pellucidum.

Tertius

Tertius parvus & tenuis adhuc dilutioris eſt coloris. Poli in lateribus oppoſitis ſunt ſiti, aſt hic æque ac quintus in eo a reliquis differt, quod totus ſit pellucidus Fig. 3.

Hi tres e Ceylona apportati, & quantum ſcio omnes ex hac inſula in Europam tranſlati, colore flaveſcente præditi fuerunt, licet magis minuſve nigri fuerit admixtum. E contrario in Braſilia (ab æquatore auſtrum verſus æque circiter diſtante, ac Ceylona in hemiſphærio boreali) reperti turmalini, alios induunt colores. Rubros, virides & cœruleos vidi, forte etiam aliter tincti occurrunt.

Quartus viridis eſt, cujus latera Fig. 4. monſtrat.

Quintus cyaneus, totus pellucidus, polos in lateribus oppoſitis poſſidet. Vide Figuram 5. Præterea rudes quoque habui, virides & cœruleos, nulla arte mutatos. Hi cryſtallos priſmaticas multorum laterum referunt, &, quod rarum eſt, non tantum angulos prominentes, ſed etiam inflexos habent, uti ex ſectione unius in Fig. A. adparet. Similem & regularem figuram non ſervant, ſed aliis plura, aliis pauciora latera. Pyramides terminales ſi adfuerint neſcio; omnes ſaltem erant abruptæ, cum in meas pervenirent manus. Omnes in eo luculenter conveniunt, quod poli extremitates columellarum occupent, quodque in hac directione, ſeu ſecundum longitudinem, perfecte ſint opaci.

Lex Fundamentalis.

III. *Cujuſvis turmalini alter polus dilatatione electricitatem adquirit poſitivam, contractione negativam; alter vero contrariam habet indolem, adeo ut contractione fiat poſitivus & dilatatione negativus.*

Dum pororum capacitates minuuntur, turmalinum in ſtatu contractionis eſſe dico. Status vero dilatationis adeſt, dum eorundem amplitudines augentur.

Sint ABN & ABLM (Fig.6.) duo media, quorum alterutrum e. g. ABLM ſit calidius. Turmalinus temperie medii AL præditus, in eo nulla electricitatis ſigna exſerit, intret vero alterum frigidius, mox pori contrahuntur & lapillus fit electricus, idque hoc modo, ut alter polus fiat +, alter —. Poſtquam vero heic differentiam caloris plene amiſerit, omnia ceſſant phænomena electrica. Si vero jam in calidius remigret, iterum fit electricus, aſt dilatatione, & quidem contrario modo, adeo ut polus in priori caſu +, heic fiat —, — autem +. Hanc vim ſimiliter retinet uſque dum pororum capacitates calori medii fuerint adaptatæ.

Hæcce propoſitio explicata adeo eſt fœcunda, ut illa poſita, omnes quotquot mihi notæ ſunt proprietates electricæ, turmalino in ſpecie competentes, ex hac fluant, corolariorum inſtar, ex theoremate fundamentali. Phænomena frictionis huc non pertinent, quippe quæ turmalino cum multis aliis communia.

IV. Quæ ex lege fundamentali prono, ut aiunt, alveo deduci queant, jam examinanda. In genere patet, nonniſi quinque variationes eſſe poſſibiles; aut enim

alter porus est	(+)	& alter	(—)	
aut - - -	(+)	- -	(+)	
aut - - -	(—)	- -	(—)	
aut - - -	(+)	- -	(±)	*
aut - - -	(—)	- -	(±)	

Hæ tamen omnes variationes tribus comprehendi possunt casibus.

(+) (—)

V. Casus primus duplici probari potest modo, & calore & frigore. Si turmalinus liquidis ebullientibus, carbonibus ardentibus, vel alio quocunque modo æqualiter calefactus, in aëre frigidiore examini subjicitur, in statu contractionis invenietur. Heic adnotasse juvat electricitatem turmalini, aqua fervida extracti, calori non esse adscribendam, nam illa vis, quæ primis momentis intra aquam generatur, mox destruitur, partim medio ambiente, partim eam ob caussam, quod cito fixum nanciscatur aquæ calorem. Extractus igitur lapillus initio nullam possidet electricitatem, sed sub voluminis contractione paucis minutis secundis comparat.

Metallis calidis impositus, e camera frigidiore in calidiorem translatus, vel quocunque alio modo volumine auctus, reperitur in statu dilatationis.

Frigore artificiali rite & prudenter adhibito, statum vel contractionis, vel dilatationis prolubitu provocavi.

(+) (+)
(—) (—)

VI. Ut turmalinus eandem ubique nanciscatur electricitatem, alter polus contrahi debet, dum alter dilatatur. Experientiam consulamus.

* Statum naturalem voco, dum nulla adsunt electricitatis signa, eumque designo ±.

Teneatur

Teneatur alter polus juxta candelæ flammam, dimidio circiter minuto, magis minuſve pro craſſitie lapilli (Fig. 7.). Si dein exploretur, uterque polus eam poſſidet electricitatem, quam polus calefactus in ſtatu contractionis adquirit. Ratio hæc eſt. Quia alter polus magis calefit, neceſſe quoque eſt, ut e flamma remotus magis contrahatur, quam alter, qui parum vel nihil caloris obtinuit. Hinc calor, quem interiores particulæ nactæ ſunt, alterum polum verſus (ſeu non calefactum) cogitur, eodem fere modo ac faber calorem fundum verſus aqua pellit, qua carbones adſpergit. Polus igitur non calefactus parvo temporis ſpatio dilatatur, dum alter contrahitur, ideoque eandem uterque monſtrat vim (§ 3.). Aſt hæc poli non calefacti dilatatio non diu poteſt durare, ideoque cito contrahi incipit, & phænomena caſus primi oriuntur.

VII. Ex hoc fundamento celebratiſſimorum phyſicorum, Wilſoni & Æpini litem componendam eſſe perſuaſus ſum. Experimentum Wilſonianum, ſectione præcedente explicatum, rite inſtitutum ſemper ſuccedit; ſi vero alter polus diutius vel fortiori, quam par eſt, exponatur igni, calor tanta copia penetrat, ut lapillo a flamma remoto, oppoſitus quoque contrahatur, non obſtante particula caloris interni illum verſus pulſi; & per ſe patet tunc prodire caſum primum. Forte tali modo Domino Æpino phænomena caſus ſecundi effugerunt, quam ſuſpicionem magnitudines quoque turmalinorum, quos adhibet phyſicus Petropolitanus, confirmant.

Carbonibus ardentibus prudenter adhibitis phænomena Londinenſia æque feliciter prodeunt.

Æpiniana tentamina fortioribus mutationibus producuntur, & iiſdem principiis luculenter congruunt.

Polus enim carbonem ardentem primo tangens, & mox vitro vel laminæ metallicæ impoſitus, ex novo contactu vehementius contrahitur, quam in aëre ejuſdem temperiei, in ratione majoris ſoliditatis, adeoque oppoſitus non poteſt non calore, illum verſus ruente, magis dilatàri. Quomodo vero tandem ſtatum contractionis induat, antea dictum eſt.

Nulla igitur eſt pugna inter experimenta Petripolitana & Londinenſia. Solus in explicatione conſiſtit diſſenſus. Ambo in caloris inæquali diſtributione cauſam quærunt. Hæc inæqualitas revera quoque adeſt, ſed ex adductis in aprico eſt, illam pro circumſtantiis protei inſtar varias induere ſpecies.

$$(+)\ (\pm)$$
$$(-)\ (\pm)$$

VIII. Ut caſus tertius prodeat, hoc eſt ut alter polus ſit vel +, vel —, alter vero nullam prodat electricitatem, polus electricus juxta legem fundamentalem contrahi vel dilatari deberet, altero manente immutato. Hoc difficulter experimento monſtratur, modo vero ſequenti quodammodo mihi ſatisfeci. Alter polus laminæ metallicæ, pede vitreo inſtructæ, imponatur, & oppoſitus induſio tegatur metallico, ex quo columella extremo rotundato ſurgit, alii ex lamina, uti Fig. 8. indicat, reſpondens. Hiſce ita præparatis, metallum candens laminæ imponatur, quo polus hanc tangens momento fere eundem caloris gradum accipit & per aliquot minuta conſervat. Hinc lamina hoc tempore vix ullam prodit electricitatem, dum interea induſium vivaciſſima dilatationis edit ſigna. Turmalino quodam rudi hoc facilius præſtavi, nam columnæ longitudo fecit; ut alterum extremum calefacere potuerim, alterius ſtatu naturali manente immutato.

IX. Allata

IX. Allata ſufficienter evincunt, phænomena electrica turmalini, quam ſatis, unicæ ſimplicique convenire legi, adeo ut proprietates, quas hucuſque phyſicorum ſolertia detexit, & forte exhaurivit, paucis comprehendi poſſint lineis. Si ubique naturæ operationes æque feliciter ad cauſſas proximas revocare liceret, prolixi phænomenorum catalogi in minimam cito redigerentur molem, effectus continentem principales. Ut huc perveniamus, hoc opus, hic labor.

X. Unicum addam experimentum, contrarias electricitates, me judice, luculenter demonſtrans. Scilicet capſulam metallicam, manubrio vitreo donatam (Fig. 9.) aqua fervida implevi, eique turmalinum injeci. Lapillus heic ſtatum dilatationis adquirit, ſed nihilo minus capſula ne minimum quidem electricitatis ſignum prodit. Si negativa, nonniſi debilior poſitivæ gradus eſſet, earum ſumma in præſenti caſu ſine dubio foret conſpicua.

XXIX. *Theory of the Parallaxes of Altitude for the Sphere, by Mr.* F. Mallet, *Professor and Astronomer at* Upsal; *Translated from the* French *by* M. Maty, *M. D. R. S. Sec.*

Upsal, October 25, 1766.

Read Nov. 20, 1766.

§ 1. LET P be = the moon's horizontal parallax, or 1 to sin. P, as the moon's distance to the radius of the terrestrial sphere, on which the spectator is supposed to be placed. Let A be the distance of the moon from the zenith, and p the parallax of altitude for the same distance. The astronomers usually compute the value of p in the following manner: let sin. p=sin. P. Sin. A, and p' being found by the tables of logarithmic sines, sin. p''= sin. P. sin. $\overline{A+p'}$ is found in like manner, p'' being assumed for the true parallax, which is not accurate.

§ 2. In order to shew this, I have given another method of computing the parallax of altitude, as exactly as may be, by means of the common tables, in the following manner. Since sin. p = sin. P sin. $\overline{A+p}$, we have sin. p = sin. P sin. A cos. p + sin. P cos. A × sin. p, or sin. p (1 − sin. P cos. A) = sin. P sin. A cos. p; hence tang. $p = \frac{\text{sin. P sin. A}}{1-\text{sin. P cos. A}}$. This formula seems a little difficult to be wrought in numbers, but it is as easy as the above one; for, supposing sin. B^2 = sin.

ſin. P coſ. A, the tables will give the angle B, and tang. $p = \frac{\text{ſin. P ſin. A}}{\text{coſ. B}^2}$, the computation of which can give no trouble. Hence it appears, that the calculus for finding the true parallax is not more difficult than that, which gives the ſaid parallax with an error, the value of which is unknown; for it is evident that the above computation for finding p'' is only an approximation, and that, to make it accurate, it would be neceſſary to carry it ſtill on by finding ſin. $p''' =$ ſin. P ſin. $(\overline{A+p''})$, and afterwards ſin. $p'''' =$ P ſin. $(\overline{A+p''})$ &c.

§ 3. I therefore think myſelf in the right to prefer my method to that hitherto uſed by aſtronomers. To confirm my opinion, I made a trial, by putting $P = 59'$ and $A = 30°$, and found $p - p'' = 0'',43$, in which the error of the uſual computation amounts to near half a ſecond; I therefore give the preference to the geometrical calculus.

§ 4. Before I quit the formula tang. $p = \frac{\text{ſin. P ſin. A}}{1 - \text{ſin. P coſ. A}}$, I muſt obſerve, that the computation of p may be executed by other methods to the ſame exactneſs. If we take coſ. 2 C = ſin. P coſ. A, we ſhall have tang. $p = \frac{\text{ſin. P ſin. A}}{2\ (\text{ſin. C})\ 2}$, and the computation of this new formula is extremely eaſy.

§ 5. The formula tang. $p = \frac{\text{ſin. P ſin. A}}{1 - \text{ſin. P coſ. A}}$, gives beſides, ſin. $p = \frac{\text{ſin. P ſin. A}}{\sqrt{1 + \text{ſin. P}^2 - 2\ \text{ſin. P coſ. A}}}$; make ſin. P = 2 coſ. D, D being a given angle, of which we may have tables ready made, and we ſhall have ſin.

$p =$

$$p = \frac{\text{fin. P fin. A}}{\sqrt{1 + 2\,\text{fin. P}\,(\text{cof. D} - \text{cof. A}}} = \frac{\text{fin. P fin. A}}{\sqrt{1 + 4\,\text{fin. P fin.}\frac{A+D}{2}\,\text{fin.}\frac{A-D}{2}}};$$ fince cof. D—cof. A $= 2$ fin. $\frac{A+D}{2}$ fin. $\frac{A-D}{2}$. This being found without any logarithmic computation, we fhall find tang. $E^2 = 4$ fin. P fin. $\frac{A+D}{2}$ fin. $\frac{A-D}{2}$, if $A > D$, and hence we may eafily compute fin. $p =$ fin. P fin. A cof. E; but if $A < D$ we fhall find cof. $F^2 = 4$ fin. P fin. $\frac{A+D}{2}$ fin. $\frac{A-D}{2}$ and hence fin. $p = \frac{\text{fin. P fin. A}}{\text{fin. F}}$.

§ 6. Similar formulæ may be found for cof. p, but as the angle p is pretty fmall, one might eafily fall into fome error by the ufual tables of logarithms. I fhall not fay what would be the amount of this error of p, having furnifhed the manner of avoiding it; but this remark has not, I think, as yet been made in aftronomical calculations; and I have found it of great confequence in computing eclipfes, where the diftances to be found are very fmall arches.

§ 7. It may moreover be obferved, that if $A = D$, fin. $p =$ fin. P fin. A; hence $p' = p$ in the fame cafe, and $p'' > p$, which feems very odd; but the moon then is below the fenfible horizon.

Theory of the apparent Diameters of the Moon.

§ 1. Firft the expreffion of horizontal diameter of the moon, or of the diameter feen at the horizon, feems to me too vague; for one ought to underftand by it the diameter feen at the center of the terreftrial

terreſtrial ſphere, rather than the apparent diameter at the horizon, which is not affected by refraction. Without this, if the one was confounded with the other, an error would ariſe for the latitude of Paris from $0'',25$ to $0'',32$.

§ 2. Let us keep the ſame denominations of P, p, and A, and call D the apparent ſemi-diameter of the moon at the centre of the ſphere, and $d =$ the apparent ſemi-diameter of the moon at the zenith diſtance $=$A. We ſhall have ſin. A : ſin. $\overline{A+p}$: : tang. D : tang. d, or if one will, ſin. A : ſin. $\overline{A+p}$: : D : d : the error not exceeding an 100th part of a ſecond.

§ 3. We had above ſin. $p =$ ſin. P ſin. $\overline{A+p}$. Hence ſin. P ſin. A : ſin. p : : (tang. D : tang. d) : : D : d, or becauſe ſin. $p = \frac{\text{coſ. } p \text{ ſin. P ſin. A}}{1 - \text{ſin. P coſ. A}}$, 1 – ſin. P coſ. A : coſ. p : : D, d, and $d = \frac{\text{D. coſ. } p}{1 - \text{ſin. P coſ. A}}$.

§ 4. Mr. Euler, in the Memoirs of the Academy of Berlin, 1747, pag. 175, makes this ſame value $= \frac{V}{1 - p^2 \text{ ſin. } b}$, and according to him, V $=$ D. M $=$ ſin. P ſin. $b =$ coſ. $\overline{A+p}$; from whence it appears, that the true value of the apparent diameter of the moon, is not more difficult to be computed than the approximated one of Mr. Euler, the exact and geometrical formula being tang. $d = \frac{\text{tang. D coſ. } p}{1 - \text{ſin. P coſ. A}}$ and that of Mr. Euler $d = \frac{D}{1 - \text{ſin. P coſ. } \overline{A+p}}$; for in both, the values of D, A and p muſt be employed.

§ 5.

§ 5. It likewiſe appears to me, that ſince $\frac{\text{coſ. } p}{1 - \text{ſin. P coſ. A}} = \frac{\text{ſin. } p}{\text{ſin. P ſin. A}}$ and therefore tang. $d = \frac{\text{tang. D ſin. } p}{\text{ſin. P ſin. A}}$, aſtronomers ought no leſs to employ this laſt formula, than any other more troubleſome, in practical computation. The ſimpleſt is tang. $d = \frac{\text{tang. D ſin. } \overline{A+p}}{\text{ſin. A}}$, upon the ſuppoſition of an exact table of the parallaxes of altitudes ready made; and I believe it will be as eaſy to compute with tangents as with arches, by means of logarithms; and therefore this ſimplification in putting arches inſtead of tangents is unneceſſary.

§ 6. To try the conſequences of this theory, I made $A = 30°$, $D = 15'$, and taking the vertical of Upſal to the terreſtrial axis for the radius of the ſphere, I found $P = 55', 10'', 3$, ſuppoſing that the axis of the earth, is to the diameter of the equator as 199 to 200, and by the formulæ tang. $d = \frac{\text{tang. D ſin. } \overline{A+p}}{\text{ſin. A}} = \frac{\text{tang. D coſ. } p}{1 - \text{ſin. P coſ. A}} = \frac{\text{tang. D ſin. } p}{\text{ſin. P ſin. A}}$, I found $d = 15', 12''.664$, but by the formula $d = \frac{\text{D coſ. } p}{1 - \text{ſin. P coſ. A}}$, I had $d = 15', 12'', 675$. and laſtly by that of Euler $d = \frac{\text{D}}{1 - \text{ſin. P coſ. } \overline{A+p}}$ we have $d = 15', 12'', 635$; from whence it appears that the error is very ſmall, but that with the ſame trouble one may avoid any error whatſoever.

§ 7. The preſent caſe did not give an error of $0'', 001$ in ſubſtituting 1 or the radius inſtead of coſ. p. Hence I conclude that $d = \frac{\text{D}}{1 - \text{ſin. P coſ. A}}$ will be

a more

a more exact formula than that of Euler $d = \frac{D}{1 - \text{ſin. } P \text{ coſ. } \overline{A+p}}$.

§ 8. By taking $d = \frac{D}{1 - \text{ſin. } P \text{ coſ. } A}$, we have $d - D = \frac{D \text{ ſin. } P \text{ coſ. } A}{1 - \text{ſin. } P \text{ coſ. } A} = \frac{D \text{ ſin. } p \text{ coſ. } A}{\text{ſin. } A \text{ coſ. } p} = \frac{D \text{ tang } p}{\text{tang. } A}$, which affords an elegant theorem, to find the increaſe of the apparent diameter of the moon.

§ 9. I have found others by the following methods. Since $\text{ſin. } A : \text{ſin. } \overline{A+p} :: \text{tang. } D : \text{tang. } d$, and $\text{ſin. } A : \text{ſin. } \overline{A+p} - \text{ſin. } A :: \text{tang. } D : \text{tang. } d - \text{tang. } D :: \text{ſin. } D \text{ coſ. } d : \text{ſin. } \overline{d-D}$; but $\text{coſ. } D = \text{coſ. } d$ without any ſenſible error, and $\text{ſin. } D \text{ coſ. } D = \frac{1}{2} \text{ ſin. } 2D$, and $\text{ſin. } \overline{A+p} - \text{ſin. } A = 2 \text{ ſin. } \frac{1}{2} p \text{ coſ. } \overline{A + \frac{1}{2}p}$, we ſhall have $\text{ſin. } \overline{d-D} = \frac{\text{ſin. } 2D \text{ ſin. } \frac{1}{2}p \text{ coſ. } \overline{A+\frac{1}{2}p}}{\text{ſin. } A}$. In the ſame manner, as I before found $\text{ſin. } p' = \text{ſin. } P \text{ ſin. } A$ and $\text{ſin. } P \text{ ſin. } A : \text{ſin. } p :: \text{tang. } D : \text{tang. } d$, hence $\text{ſin. } p' : \text{ſin. } p - \text{ſin. } p' :: \text{ſin. } p' : 2 \text{ ſin. } \frac{p-p'}{2} \text{ coſ. } \frac{p+p'}{2} ::$

$$\text{ſin. } 2D : \text{ſin. } \overline{d-D} = \frac{\text{ſin. } 2D \text{ ſin. } \frac{\overline{p-p'}}{2} \text{ coſ. } \frac{\overline{p+p'}}{2}}{\text{ſin. } P \text{ ſin. } A}$$

§ 10. Laſtly let L = the diſtance of the moon from the center of the ſphere, l its radius, that of the ſphere being $= 1$, we have $1 : L :: \text{ſin. } P : 1$ and $L : l :: 1 : \text{tang. } D$ or $1 : l :: \text{ſin. } P : \text{tang. } D = l \text{ ſin. } P$; hence $l = \frac{\text{tang. } D}{\text{ſin. } P}$ being once found, ſince $\text{ſin. } A : \text{ſin. } \overline{A+p} :: \text{tang. } D : \text{tang. } d$, and $\text{ſin. } \overline{A+p} : \text{ſin. } p :: 1 : \text{ſin. } P$, we ſhall have $\text{ſin. } A : \text{ſin. } p :: \text{tang. } D : \text{ſin. } P \text{ tang. } d :: l : \text{tang. } d = \frac{l \text{ ſin. } p}{\text{ſin. } A}$. I found the logarithm of $l =$

 9.4343965

9.4343965 at Upſal, by putting 10 for that of the radius of the ſphere determined as before.

F. Mallet.

XXX. *A Catalogue of the* Fifty Plants *from* Chelſea Garden, *preſented to the* Royal Society *by the worſhipful Company of Apothecaries, for the Year* 1765, *purſuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. et Soc. Reg. nuper Præſes: By* William Hudſon, *Societatis Regiæ & clariſſ. Societatis Pharmaceut.* Lond. *Soc. Hort.* Chelſean. *Præfectus et Prælector Botanic.*

Read Nov. 20, 1766.

2151 ACHYRANTHES lanata, caule proſtrato, ſpicis ovatis lateralibus, calycibus tomentoſis. Lin. Sp. pl. 296. Mill. Dict. tab. 11. fig. 1.

Amaranthus Indicus verticillatus albus, foliis lanugine incanis. Pluk. alm. 27. tab. 79. f. 8.

2152 Andrachne procumbens herbacea. Lin. Sp. pl. 1439.

Telephoides Græcum humifuſum flore albo. Tourn. cor. 50. Dill. Hort. Elth. 377. tab. 282 f. 364.

2153 Bryonia Africana, foliis palmatis quinquepartitis utrinque lævibus: laciniis pinnatifidis. Lin. Sp. pl. 1438.

Bryonia

Bryonia Africana laciniata, tuberosa radice, floribus herbaceis. Herm. parad. 107. tab. 108.

2154 Carthamus corymbosus, floribus corymbosis numerosis. Lin. Sp. pl. 1164.

Chamæleon niger. Dalech. hist. 1454.

Chamæleon niger umbellatus, flore cœruleo hyacinthino. Bauh. pin. 38.

2155 Ceanothus Americanus, foliis trinerviis. Lin. Sp. pl. 284. Mill. Dict. tab. 86.

Euonymus Novi Belgii, corni feminæ foliis. Comm. hort. Amst. 1. p. 167. tab. 86. Raj. Dendr. 69.

Euonymus, jujubinis foliis, Caroliniensis, fructu parvo, fere umbellato. Pluk. alm. 139. tab. 28. f. 6.

2156 Chrysocoma, Coma aurea, fruticosa, foliis linearibus, rectis glabris. Lin. Sp. pl. 1177.

Conyza Æthiopica, flore bullato aureo, pinastri brevioribus foliis læte viridibus. Pluk. alm. 400. tab. 327. f. 2.

2157 Coronilla juncea, fruticosa, foliis quinatis ternatisque lineari-lanceolatis subcarnosis obtusis, Lin. Sp. pl. 1047.

Colutea caule genistæ fungoso. Bauh. hist. 1. p. 383.

Polygala major massiliotica. Bauh. pin. 246.

2158 Draba, muralis, caule ramoso, foliis cordatis dentatis amplexicaulibus. Lin. Sp. pl. 897. Hudf. Fl. angl. 243.

Bursa pastoris major loculo oblongo. Bauh. pin. 108. pr. 50. tab. 50.

2159 Galium verrucosum, foliis senis lanceolatis serrato-aculeatis, pedunculis trifloris, fructibus verrucosis.

Aparine; femine coriandri facchardti. Park. theatr. 567. Rand. Ind. Hort. Chelf. 19.

2160 Gypfophylla pilofa, foliis lanceolatis trinerviis amplexicaulibus, caule pilofo, floribus folitariis, pedunculis filiformibus longiffimis.

2161 Gypfophylla proftrata, foliis lanceolatis lævibus, caulibus diffufis, piftillis corolla campanulata longioribus. Lin. Sp. pl. 581.

2162 Hedera quinquefolia, foliis quinatis ovatis ferratis. Lin. Sp. pl. 292.

Edera quinquefolia Canadenfis. Corn. Canad. 99. tab. 100.

2163 Hypericum, Androfæmum, floribus trigynis, pericarpiis baccatis, caule fruticofo ancipiti. Lin. Sp. pl. 1102. Hudf. Fl. Angl. 291.

Hypericum maximum, Androfæmum vulgare dictum Raj. Syn. 343.

Androfæmum maximum frutefcens. Bauh. pin. 208.

2164 Hypericum hirfutum, floribus trigynis, calycibus ferrato-glandulofis, caule tereti erecto, foliis ovatis fubpubefcentibus. Lin. Sp. pl. 1105. Hudf. Fl. Angl. 291.

Androfæmum hirfutum. Bauh. pin. 280.

2165 Hypericum montanum, floribus trigynis, calycibus ferraturis glandulofis, caule tereti erecto glabro, foliis ovatis. Lin. Sp. pl. 1105. Hudf. Fl. Angl. 291.

Hypericum elegantiffimum non ramofum, folio lato Bauh. hift. 3. p. 383.

2166 Hypericum fupinum, floribus trigynis axillaribus folitariis, caulibus ancipitibus proftratis filiformibus,

formibus, foliis glabris. Lin. Sp. pl. 1105. Hudſ. Fl. Angl. 290.

Hypericum minus ſupinum glabrum. Bauh. pin. 279.

2167 Hypericum pulchrum, floribus trigynis, calycibus ſerratoglanduloſis, foliis cordatis glabris, caule tereti. Lin. Sp. pl. 1106. Hudſ. Fl. Angl. 290.

Hypericum minus erectum. Bauh. pin. 279.

Hypericum pulchrum Tragi. Bauh. hiſt. 3. p. 383. Raj. Syn. 343.

2168 Hypericum elodes, floribus trigynis, foliis rotundis tomentoſis, caule repente. Hudſ. Fl. Angl. 292.

Hypericum floribus trigynis, caule tereti foliiſque villoſis ſubrotundis. Lin. Sp. pl. 1106.

Aſcyrum ſupinum villoſum paluſtre. Bauh. pin. 280.

2169 Hypericum Balearicum, floribus pentagynis, caule fruticoſo, foliis ramiſque cicatriſatis. Lin. Sp. pl. 1101. Mill. Ic. tab. 54.

Myrto-Ciſtus Pennæi. Cluſ. hiſt. 1. p. 68.

2170 Lepidium Bonarienſe, floribus diandris tetrapetalis, foliis omnibus multifidis. Lin. Sp. pl. 901.

Thlaſpi Bonarienſe multiſciſſum, flore inviſibili. Dill. Elth. 281. tab. 286. f. 370.

2171 Lotus edulis, leguminibus ſubſolitariis gibbis incurvis. Lin. ſp. pl. 1090.

Lotus pentaphyllos, ſiliqua cornuta. Bauh. pin. 332.

Lotus edulis Cretica. Raj. hiſt. 967.

2172 Lotus hirſutus, capitulis hirſutis, caule erecto

hirto

hirto, leguminibus ovatis. Lin. Sp. pl. 1091.
Lotus pentaphyllos siliquosus villosus. Bauh. pin. 372.

2173 Oldenlandia biflora, pedunculis bifloris petiolo longioribus, foliis lanceolatis. Lin. Sp. pl. 174. Fl. Zeyl. 68.

2174 Oxalis stricta, caule ramoso erecto, pedunculis umbelliferis. Gron. virg. 161. Lin. Sp. pl. 624.
Trifolium acetosum corniculatum luteum majus rectum Indicum. Hist. Ox. 2. p. 184. f. 2. tab. 17. f. 3.

2175 Physalis, angulata, ramosissima, ramis angulatis glabris, foliis dentatis ovatis. Lin. Sp. pl. 262.
Solanum vesicarium Indicum. Bauh. pin. 166.
Alkekengi Indicum glabrum, chenopodii folio. Dill. Elth. 15. tab. 12. f. 12.

2176 Physalis, pubescens, ramosissima, foliis villoso-viscosis, floribus pendulis. Lin. Sp. pl. 262.
Solanum vesicarium Virginianum procumbens annuum, folio lanuginoso. Hist. Ox. 3. p. 527. f. 13. tab. 3. f. 24.

2177 Physalis curassavica, caule fruticoso, foliis ovatis tomentosis. Lin. Sp. pl. 261.
Solanum vesicarium curassavicum, solano antiquorum simile, foliis origani subincanis. Hist. Ox. 3. p. 527. Pluk. Alm. 352. tab. 111. f. 5.

2178 Passiflora laurifolia, foliis indivisis ovatis integerrimis, petiolis biglandulosis, involucris dentatis. Lin. Sp. pl. 1356.

Passiflora

Paſſiflora arborea, laurinis foliis, Americana. Pluk. alm. 282. tab. 211. f. 3.

2179 Paſſiflora veſpertilio, foliis bilobis baſi rotundatis glanduloſiſque: lobis acutis divaricatis, ſubtus punctatis. Lin. Sp. pl. 1357.

Granadilla bicornis, flore candido, filamentis intortis. Dill. Elth. 164. tab. 137. f. 164.

2180 Reſeda alba, foliis pinnatis, floribus tetragynis, calycibus ſexpartitis. Hort. Upſ. 149. Lin. Sp. 645.

Reſeda maxima. Bauh. pin. 100.

Reſeda foliis calcitrapæ; flore albo. Moriſ. Hort. Bleſſ.

2181 Reſeda undata, floribus trigynis tetragyniſque, calycibus quinque partitis, foliis pinnatis undatis. Lin. Sp. pl. 644.

Reſeda minor, foliis inciſis. Barr. rar. 78. tab. 588.

2182 Serratula ſcarioſa, foliis lanceolatis integerrimis, calycibus ſquarroſis pedunculatis obtuſis laterationibus. Lin. Sp. pl. 1147.

Eupatorio affinis Americana bulboſa, floribus ſcarioſis, capitulis contextis. Pluk. alm. 142 tab. 177. f. 4.

2183 Sibbaldia procumbens foliolis tridentatis. Lin. Sp. pl. 406. Hudſ. Fl. Ang. 118.

Pentaphylloides pumilla foliis ternis ad extremitates trifidis. Raj. Syn. 256.

Fragariæ ſylveſtri affinis planta, flore luteo. Sibb. pr. hiſt. natur. Scot. p. 11. pag. 25. tab. 6. f. 1.

2184 Silene quinquevulnera, petalis integerrimis

 ſubrotundis,

ſubrotundis, fructibus erectis alternis. Lin. Sp. pl. 595.

Lychnis ſylveſtris lanuginoſa minor. Bauh. pin. 206.

Lychnis hirſuta, flore eleganter variegato. Raj. hiſt. 997.

2185 Silene Valleſia, caulibus ſubunifloris decumbentibus, foliis lanceolatis tomentoſis longitudine calycis. Lin Sp. pl. 603.

Lychnis maritima pinguis e Corſica. Bocc. muſ. tab. 34.

2186 Solanum Bonarienſe, caule ſubfruteſcente ſubinermi, foliis cuneiformibus ſinuato-repandis. Lin. Sp. pl. 264.

Solanum Bonarienſe arboreſcens, papas floribus. Dill. Elth. 364. tab. 272. f. 351.

2187 Solanum Indicum, caule aculeato fruticoſo, foliis cuneiformibus angulatis ſubvilloſis integerrimis, aculeis utrinque rectis. Lin. Sp. pl. 268. Fl. Zeyl. 94.

Solanum Indicum ſpinoſum, flore boraginis. Robert. Ic. 28. Dill. Elth. 362. tab. 270. f. 349.

2188 Sophora alopecuriodes, foliis pinnatis: foliolis numeroſis oblongis villoſis caule herbaceo. Lin. Sp. pl. 533.

Ervum Orientale alopecuriodes perenne, fructu longiſſimo. Tourn. cor. 27. Dill. Elth. 136.

2189 Theligonum. Lin. Sp. pl. 1411.

Cynocrambe Dioſcoridis. Bauh. pin. 122. pr. 59. tab. 59.

2190 Trigonella polycerata, leguminibus ſubſeſſilibus congeſtis erectis ſubrectis longis linearibus, pedunculis

pedunculis communibus muticis. Lin. Sp pl. 1093.

Fœnum Græcum ſylveſtre alterum polyceration. Bauh. pin. 348.

2191 Trygonella corniculata, leguminibus pedunculatis congeſtis declinatis ſubfalcatis, pedunculis communibus longis ſubpinoſis, caule erecto. Lin. Sp. pl. 1094.

Melilotus, corniculis reflexis, major. Bauh. pin. 331.

2192 Trigonella ſpinoſa, leguminibus ſubpedunculatis congeſtis declinatis ſubfalcatis, pedunculis communibus ſpinoſis breviſſimis. Lin. Sp. pl. 1094.

Fœnum græcum ſylveſtre polyceration Creticum majus. Breyn. Cent. 79. tab. 33. f. 1.

2193 Trygonella Monſpeliaca, leguminibus ſeſſilibus congeſtis arcuatis divaricatis inclinatis brevibus, pedunculis molliter mucronatis. Lin. Sp. pl. 1095.

Fœnum græcum ſylveſtre polyceration minus Monſpeliacum. Breyn. Cent. 80. tab. 33. f. 2.

2194 Valantia hiſpida, floribus maſculis trifidis hermaphroditici germini hiſpido inſidentibus. Lin. Sp. pl. 1490.

Galium floribus maſculis trifidis omnibus plantæ partibus hiſpidis. Zinn. goet. 233.

2195 Valantia muralis, floribus maſculis trifidis hermaphroditico germini glabro inſidentibus. Lin. Sp. pl. 1490.

Rubeola echinata ſaxatilis. Bauh. pin. 334.

Cruciata muralis minima Romana. Col. ecphr. 1. p.

p. 298. tab. 297. hist. Ox. 3. p. 328. s. 9. tab. 21. f. 2.

2196 Valantia articulata, floribus masculis quadrifidis, pedunculis dichotomis nudis, foliis cordatis. Lin. Sp. pl. 1491.

Cruciata orientalis latifolia erecta glabra. Tourn. Cor. 4.

2197 Valantia cruciata, floribus masculis quadrifidis, pedunculis diphyllis. Lin. Sp. pl. 1491. Hudſ. Fl. Angl. 375.

Cruciata hirsuta. Bauh. pin. 335.

2198 Veronica, Chamædrys, racemis lateralibus, foliis ovatis sessilibus rugosis dentatis, caule debili. Lin. Sp. pl. 17. Hudſ. Fl. Angl. 5.

Chamædrys spuria minor rotundifolia. Bauh. pin. 249.

2199 Veronica montana, racemis lateralibus paucifloris, calycibus hirsutis, foliis ovatis rugosis crenatis petiolatis, caule debili. Lin. Sp. pl. 17. Hudſ. Fl. Angl. 5.

Veronica chamædryoides, foliis pediculis oblongis insidentibus. Raj. Syn. 281.

Chamædryi spuriæ affinis rotundifolia scutellata. Bauh. pin. 249.

2200 Vitis arborea, foliis supra decompositis: foliolis lateralibus pinnatis. Lin. Sp. pl. 294.

Frutex scandens, petroselini foliis, Virginianus claviculis donatis. Pluk. Mant. 85. tab. 412. f. 2.

XXX. *Ob-*

XXX. *Obſervations on the Eclipſe of the Sun of* Auguſt 5, 1766, *made at* Colombes, *the Obſervatory of the Marquis of* Courtenvaux, 20° *Weſt of the Royal Obſervatory at* Paris, *and in Lat.* 48°55′28″. *By* M. Meſſier, *Aſtronomer to the Marine of* France, *of the Royal Academy of Sciences at* Paris, *and F. R. S. Tranſlated by* M. Maty, *M. D. Sec. R. S.*

Read Nov. 26, 1766.

THE Marquis of Courtenvaux having deſired me to obſerve the eclipſe of the 5th of Auguſt 1766, in his Obſervatory, I got thither ſome days before the obſervation, in order to verify the clocks, by correſponding altitudes of the ſun, and by its tranſits, with an inſtrument placed in the plane of the meridian. It is a common refractor of 5 feet focus, which does not vary a ſecond from the plane of the meridian. The day of the eclipſe, and the next day, I took a great many correſponding altitudes, and likewiſe obſerved the ſun with the tranſit inſtrument. The ſky was perfectly clear at the time of theſe obſervations, as well as during the eclipſe. The clock which I made uſe of was adjuſted to the mean time; it goes very regularly. For the obſervation of the eclipſe, I employed an excellent Gregorian teleſcope of two feet focus, conſtructed in England by the celebrated Mr. Short. The tube was

mounted on a braſs parallactic machine exactly divided. There likewiſe was a micrometer with ſilk threads adapted to this inſtrument, which was moveable every way, in ſo much that it was eaſy to place it according to the ſun's parallel, and to meaſure with great facility the ſolar diameter, as well as the diſtances of the cuſps, and the parts of the ſun which remained uncovered. To make the obſervation of the beginning, I had determined, by means of the micrometer, the point of the ſolar limb, where the contact was to happen. This was a little lower than the ſun's diameter parallel to the equator; the point of contact was not diſtant from it above 2′ 30″, and the time was exactly $5^h\ 43'\ 50''$ true time. Mr. Jeaurat, who obſerved in the ſame obſervatory with myſelf, judged the beginning $3\frac{1}{2}''$ later by a refractor of 5 feet focus.

Here follows the reſult of my obſervations:

Time by the Clock.	Tr. Tim.	Parts of the Microm.	Diſt. of the Cuſps.	Light Parts of the Sun.	Diameter of the Sun.
H. M. S.	H. M. S.		M. S. T.	M. S. T.	M. S. T.
0 3 29	0 0 0	1982			
5 47 14½	5 43 50	Beginning of the Eclipſe to a Second			
5 53 0	5 49 35½	713	11 22 56		
5 58 0	5 54 35½	938	14 58 27		
6 1 51	5 58 26½	1660		26 30 0	
6 5 4	6 1 39½	1151	18 22 28		
6 8 0	6 4 35½	1986			31 42 16
6 11 0	6 7 35½	1288	20 33 42		
6 14 0	6 10 35½	1423 ½		22 43 27	
6 18 13	6 14 48½	1413	22 23 25		
6 20 54	6 17 29½	1230 ½		19 38 36	
6 27 7	6 23 43	1248		19 55 23	
6 30 1	6 26 37	1513	24 9 12		
6 36 0	6 32 36	1534	24 29 19		
6 38 23	6 34 59	1192		19 1 44	
6 42 9	6 38 45	1988			31 44 11
6 45 34	6 42 10	1514 ½	24 10 38		
6 57 25	6 54 1	1399	22 20 0		
7 4 26	7 1 2	1289	20 34 39		
7 8 36	7 5 12	1183	18 53 7		
7 11 54	7 8 30	1053	16 48 35		
7 16 43	7 13 19	905 ½	14 27 18		
7 22 22	7 18 58	643	10 15 53		
	7 22 36				

The ſun diſappears behind the trees of the park; but very near the horizon, and the eclipſe is almoſt at an end.

XXXI. *A Letter from the Prince* de Croy *to the Earl of* Morton, *President of the R. S. containing the Observations of the Eclipses of the Sun of the 16th of* August 1765, *and of the 5th of* August 1766, *made at* Calais, *together with some Remarks on the first of these Eclipses: Translated from the* French, *by* Mathew Maty, *M. D. Sec. R. S.*

Calais, August 9, 1766.

My Lord,

Read Nov. 27, 1766.

I Have the honour to send your lordship, according to your desire, the curious observation, which I made of the eclipse of last year; and likewise inclose with it the observation we just now have been making, and which, though less curious, (because this part of the moon has neither pits nor mountains), confirms the former with regard to the height of the cusps, or that small elevation which is seen against the sun's cusps. I do not send the figure of the latter, because it is nearly the same thing, excepting that it was, as you know, towards the lower part of the sun. I think I have seen enough to conclude, that the atmosphere of the moon extends to at least double the height of the highest mountains

in it, and that it has ſome dry cavities, which ſink below the diſk. Theſe two remarks, having been thought curious by Mr. Le Monier, and Mr. Caſſiny, I have taken the liberty to communicate them to you; and have the honour to be,

My Lord,

Your Lordſhip's

moſt obedient humble Servant,

Le Prince de Croy.

Obſervation of the Eclipſe of the Sun, of the 16th of August 1765, *at* Calais, *near the Steeple, in the ſame Place, where the central and annular Eclipſe was obſerved.*

Beginning - - - -	3^h	$50'$	$46''$*
End - - - - -	5	8	$17\frac{1}{2}$
The duration, of which I am very certain	1	17	$31\frac{1}{2}$

The greateſt magnitude was of $2^{dig.}\ 50^{min.}$

The points of ingreſs and egreſs are marked as they appeared to us in the three refracting teleſcopes, two of which had very nice micrometers, and in the large refractor.

* This might have happened 1″ ſooner, viz. at 45″, on account of the uncertainty of the ingreſs. I, however, think the former determination juſt, becauſe I, at that inſtant, was pointing with the great refractor towards the ſpot, where the moon entered, and I was ſtruck with the ſuddenneſs of the ingreſs of the penumbra.

refractor. These, as well as the size of the cusps and the number of digits, were verified by Mr. de Fourcroy, chief engineer, who was so good as to assist us, and by Mr. Mouron, who assisted Mr. Blondeau in the observation of the annular eclipse.

There apppeared hardly any sign of an atmosphere, except against the cusps of the sun, which seemed something bigger at the two sides, which touched the moon. There is no indication of one any where else, unless that the disk seems not so well defined at the ingress and egress. These two remarks would induce me to believe there is a small atmosphere; and this is the subject of a separate memoir.

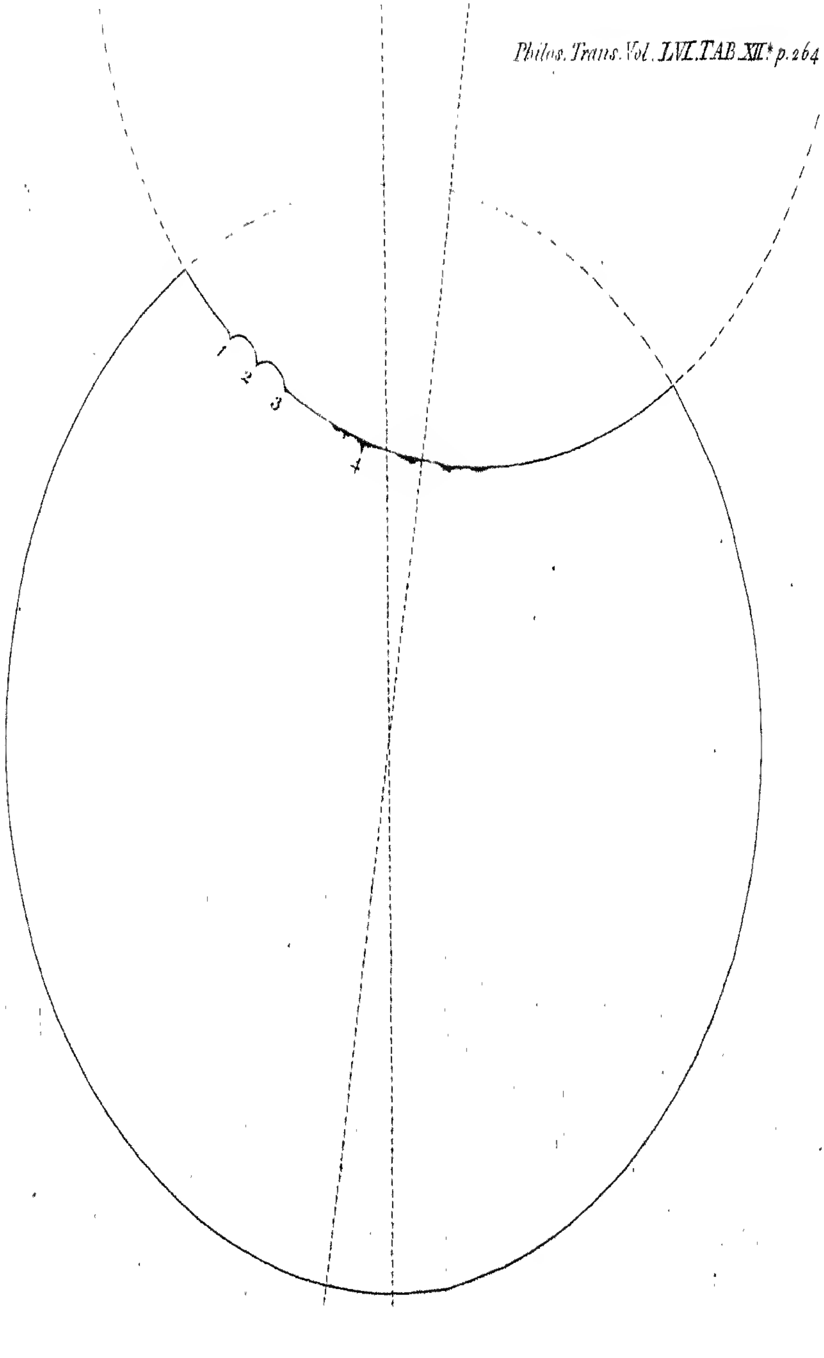
I saw very distinctly with the reflector of 4 feet 3 inches (which answers a common telescope of 70 or 80 feet) and with the acromatic telescope, several mountains, and particularly five, which I have delineated (see TAB. XII), besides some inequalities. I always saw them in their fixed places, whatever motion was given to the telescope.

N° 1. Emerged from the sun's left limb or cusp at - - - -	4^h	$32'$	$6''$
2. - - - - -	4	35	18
3. - - - - -	4	38	6

These seemed to have their summits at about an equal distance from one another.

The mountain N°. 4. which was the highest and well defined, made its exit at $4^h\ 45'\ 0''$; it was accompanied with a smaller but well defined one a little on the left. On the right hand several inequalities remained; but the right limb of the moon had none, and was smooth and perfectly well defined.

6 We


1
2
3
4

We all four very well ſaw theſe mountains, and we certify their exiſtence. The three firſt ſeemed to have hollows between them, which penetrate a little into the moon; all the reſt projected out of the diſc.

The clock was verified by the meridian the ſix following days. The whole is referred to the meridian and point of Calais; and it appears that the time marked for the new moon of that day requires a rectification. Signed,

Prince de Croy, De Fourcroy, chief Engnieer,
Garnier, Engineer: Mouron.

Remarks on the Eclipſe of the 16th of August 1765 *at* Calais, *with regard to the Atmoſphere, and the Mountains ſeen in profile.*

The Atmoſphere.

Upon mature conſideration, I think the moon's atmoſphere perceptible, ſee Fig. 1. 1. Becauſe the two ſmall elevations, which I conſtantly ſaw of the ſun's limb, towards the cuſps, is eaſily diſtinguiſhed in the large teleſcope, through which the other parts appeared very exactly defined. It is true that the ſun appears equally bright at the place of this ſmall elevation, but the atmoſphere cannot affect its brightneſs, and can only ſomewhat raiſe the rays, which pervade or ſlide over a denſer medium.

2. Becauſe this elevation is gradual, riſing higher towards the moon, according to the degree of the denſity of its atmoſphere.

 3. Becauſe

3. Becauſe this elevation of the ſun againſt the cuſps ſeemed to me a little higher than the mountain N°. 4. which jetted out the moſt on a ſide view.

4. Becauſe the penumbra, which I obſerved at the ingreſs and egreſs, and which certainly is much leſs defined than towards the middle, is very probably occaſioned by the mixture of the rays, which are refracted in paſſing through the lunar atmoſphere. This appears to me more likely than that it ſhould be cauſed by the inflexion of the light, from the globe of the moon; becauſe ſuch inflected light ſliding upon the globe ought rather to leſſen its dark limb; whereas, on the contrary, the penumbra, which was ſufficiently thick, ſerved, as we all obſerved, to inlarge that limb.

That the teleſcope was clear, appeared from the ſpots of the ſun appearing exactly defined. A parcel of theſe was ſeen ſomewhat below the middle on the right, and above it on the left.

The Mountains.

The moſt ſurpriſing circumſtance to me, as I obſerved to the gentlemen preſent, and made a ſketch of, after half an hour's continual attention, was that of the mountains ſeen in profile, and perfectly well defined; N°. 4. in particular with the ſmall elevation on the left, had ſeveral inequalities and protuberances, which I have drawn exactly enough, though with a pencil not ſufficiently ſharp; I could have been more correct as to particulars, had I had a drawing pen*.

* Mr. Short told me in the month of July 1766, in London, that he had had a ſide view of theſe mountains with his teleſcopes of 8 and 12 feet.

The

The three ſummits of N°. 1, 2, and 3, were very diſtinct, and eſpecially the hollows between them, ſurprizing on account of their depth, for they made cavities within the circle of the diſc. I am no longer aſtoniſhed at the luminous points, which are ſeen ſtarting from the creſcent; but I wonder that no notice ſhould hitherto have been taken of theſe mountains ſeen ſideways in the eclipſes of the ſun: a four feet teleſcope well fixed is neceſſary to obſerve them diſtinctly. Large refracting teleſcopes cannot ſerve for that purpoſe, as it is difficult to prevent intirely their ſhaking. Happily we had none of them; the air was clear, the wind to the north, and I often made the eclipſe move from the top to the bottom of the field of the teleſcope, in order to ſee whether any ſpots in the glaſs did occaſion this phænomenon. But the indentings were ſtill the ſame, and equally well defined; and when their place was known, they were likewiſe ſeen through the other teleſcopes. It is only by ſight that I have marked the place and diſtances of theſe mountains, as the great teleſcope had no wires; but, to remedy this inconveniency, I exactly obſerved the inſtant of the egreſs of the four chief eminences, whence their poſition might be aſcertained: it may be, this ſide of the moon is one of the leaſt mountainous parts.

In delineating the mountain, N°. 4, whoſe figure was ſo exactly defined, I thought of the atmoſphere; but no trace of that is ſeen in that caſe. This, however, in my opinion, does not afford a ſufficient reaſon for denying it, becauſe the atmoſphere, in that poſition,

 muſt

muſt be, on all ſides, pervaded by the ſolar rays, which muſt render it inviſible to our ſight. It is therefore only againſt the cuſps, and that at the ingreſs and egreſs, that this atmoſphere is to be ſearched for, with very large inſtruments, and ſufficiently well fixed.

Prince de Croy.

N. B. I am inclined to believe, that there is no water in the moon, for the two cavities, which penetrated within the diſk continued concave to the bottom, whereas if there had been there any water, the bottom ought to have been convex. The mountains in the moon muſt be very high and hollow; and that is the reaſon of their white circle. If ever Tycho could be ſeen edge ways, it would make a beautiful appearance.

XXXII. *Obſervation of the Eclipſe of the Sun the 5th of Auguſt* 1766, *near the Courgain at* Calais.

Read Nov. 27, 1766.

BEGINNING	-	5^h 39^m 9^s
End	- - -	7 19 13

The ſun did not ſet in the ſea till about 14 minutes afther the end of the eclipſe; that is, at leaſt 10 minutes later than the almanac makes it to do at Paris. His lower limb touched the ſea at 7^h 23^m in thick vapours, which made me prefer the ſetting of this lower limb rather than the centre. It is eaſy to conclude the difference, which ſhould reſult from the different ſituations of Paris and Calais.

No

No inequalities nor mountains were diſcernable, nor could any be ſeen on that part of the moon.

The elevation of the light of the ſun's cuſps was but ſmall at the middle of this eclipſe (about as much as in the laſt eclipſe) enough, however, to indicate an atmoſphere, as in my laſt obſervation *; but the air was charged with ſtrong vapours, which hindered me from ſeeing it ſo diſtinctly as I then did.

As for the reſt, the weather was fine, calm and hot.

* Meſſ. Mouron and Rigaut ſaw it alſo, as well as I, in my great teleſcope.

Le Prince de Croy.

XXXIII. *An Account of the Extraction of three Inches and ten Lines of the Bone of the upper Arm, which was followed by a Regeneration of the bony Matter; with a Description of a Machine made use of to keep the upper and lower Pieces of the Bone at their proper Distances, during the Time that the Regeneration was taking Place; and which may also be of Service in Fractures happening near the Head of that Bone. By Mr.* Le Cat *Professor of Anatomy and Surgery at* Rouen, *Member of several Academies, and F. R. S. Translated from the* French *by* J. O. Justamond, *Surgeon to the First Troop of Horse Grenadier Guards.*

Read Nov. 27, 1766.

IN the year 1751, I communicated to the academy at Rouen the case of Charles Lehee, a child of three years old, of the village of Pitre, &c. from whom I had extracted an entire tibia, exostosed and carious in its whole extent, between the two articulations, which had remained sound: this great deficiency of bony substance was entirely supplied again by nature, and the patient acquired a new tibia, much firmer than that which he had lost. The advantages which must necessarily accrue to mankind, and the credit which surgery must acquire from observations of this sort, would induce us to hope that instances of them might become more frequent;

quent; and that the professors of the art might be thereby encouraged to attempt the preservation of limbs in all cases, where there should appear the least probability of bringing about this kind of regeneration. The observation, which I now offer to the public, is a farther proof of the powers, which nature is capable of exerting in these cases, when assisted by art. The quantity of bone regenerated is not indeed so considerable as in the preceding case; but the different age of the patient, and many other circumstances, render it equally curious and useful, and have induced me to think this account worthy to be presented to the Royal Society.

Francis Romain, called La Joye, of the village of Routot, &c. an invalid, and formerly a foot-soldier in the regiment of Languedoc, aged 41 years, received, at the battle of Rocou, a gun-shot wound in the left arm, about two fingers breadth below the head of the bone of the upper arm, which had been considerably shattered in this place by the ball.

Mr. Bousselard, his surgeon, says, he found the situation of the wound too high to risk amputation. After seven months attendance however, the patient appeared to be cured; he was put upon the list of invalids, and stationed with one of those companies at Dieppe.

Encouraged by good health, he ventured to undertake the laborious business of a ship-wright; but the great fatigue, which this employment was attended with, produced abscesses in the arm which had been lately healed; and he was admitted into our hospital in the year 1755. I immediately made free incisions and counter-openings in the parts which con-

tained the matter, and extracted ſome ſplinters of bone; I then applied a proper bandage, and after the ſeparation of ſeveral bony fragments, the cure was compleated, at leaſt a good cicatrice was formed.

He returned to his work, which was then going forward at Rouën, and was employed in carrying wood for the conſtruction of flat-bottomed boats. His limbs in general were ſtrong enough to ſupport theſe loads; but his left arm, in which he had received the wound, was of little uſe to him, being ſhorter and weaker than the other.

On the 15th of March, 1760, being ſeized with a pleuriſy and peripneumony, he was again brought into our hoſpital. After his recovery from this diſeaſe, an abſceſs was formed in the injured arm, which made an opening for itſelf in the fore-part, larger than a bullet. The arm was deprived of all motion, ſtrength, and connection; and the callus of the former fracture appeared to be entirely deſtroyed by this freſh accident. In this ſtate of the caſe, the patient being brought from the infirmary into the ward deſigned for wounded perſons, I paſſed my probe into the wound, and found the bone of the arm bare, and carious to a very great extent: the middle of this carious part was rotten and totally deſtroyed throughout its whole ſubſtance. Anodine cataplaſms were applied, to abate the inflammation and ſwelling, which attended the ulcer.

On the 15th of April I began to put in execution the plan which I had fixed upon for his cure; the firſt intention of which was, to lay bare the carious part of the bone in its whole length, which was rather

more

more than three inches. The wound was then filled with pads of lint, and the ſecond operation deferred to the next day.

It was the opinion of the by-ſtanders, that the arm ſhould be taken off at the ſhoulder-joint; but the great danger attending this kind of amputation deterred me from performing it, and induced me more particularly to conſider, whether it might not be poſſible to ſave the limb.

The inſtance of Charles Lehee had ſufficiently convinced, me that bones have the power to regenerate: it muſt indeed be allowed as a favourable circumſtance to the vegetation of the bone, that Lehee was a child; but, although this patient was an adult, I conſidered that we knew not at what age nature had put a ſtop to this regenerative faculty, and that therefore no argument could be deduced from experience to prevent the expectation of the like ſucceſs in the preſent caſe.

Theſe conſiderations determined me, and on the 16th of April I performed the operation, by ſeparating the upper and lower parts of this carious bone from their connections with the ſound parts, by methods, which every operating ſurgeon will readily conceive. I meaſured the diſtance between the end of the bone left at the upper part, and the blade of the ſaw at the lower, and found it to be juſt three inches and ten lines.

The cavity was then filled with proper dreſſings; and the form of the arm, as well as its natural length, preſerved by an inſtrument calculated to anſwer theſe intentions; the deſcription of which I thought more

 particularly

particularly deſerving the attention of the Society, as it may be of ſervice in many caſes different from this.

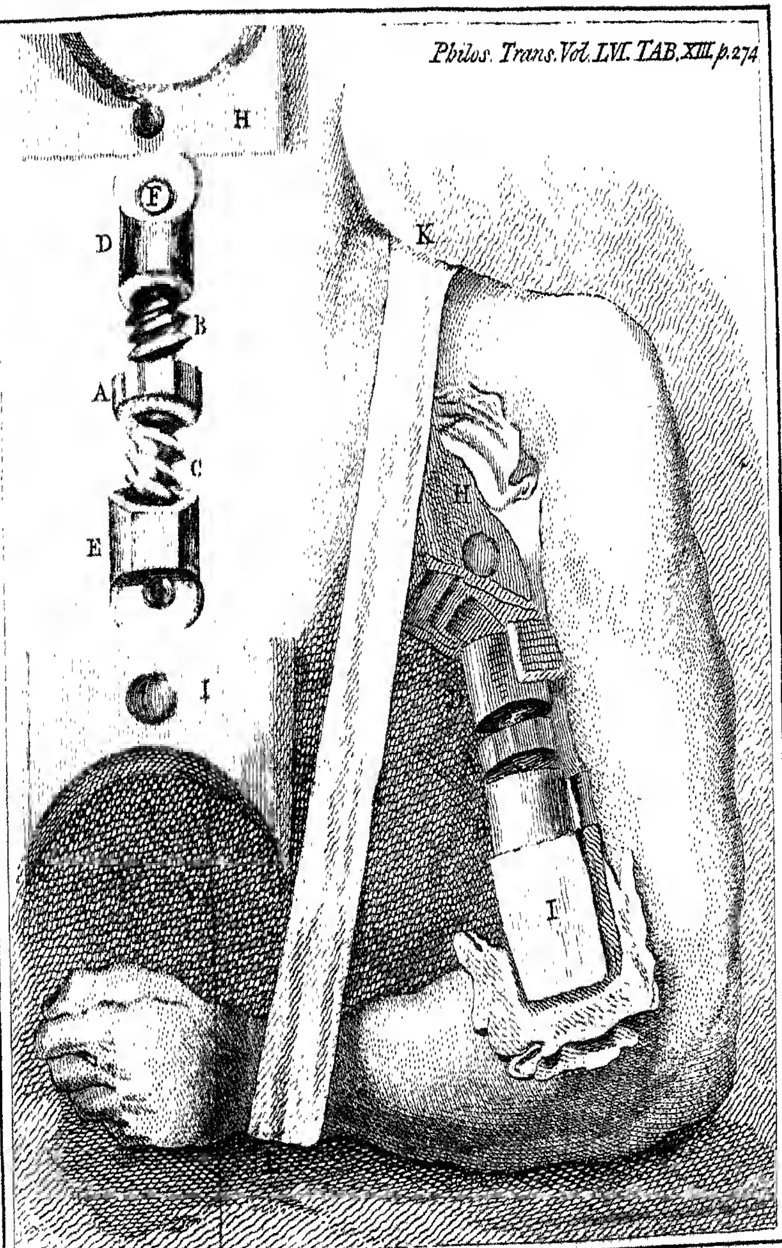
Tab. XIII. A B C is a double ſcrew, the turns of which are in contrary directions; theſe ſcrews are moved by the handle A, in ſuch a manner that, when each of them, B C, is ſcrewed into its correſpondent worm D E, one motion of the handle A brings the two worms D E nearer together; and a contrary motion ſets them at a greater diſtance.

I invented this inſtrument about fifteen years ago, to compreſs the wounds of thoſe, who had been cut for the ſtone, ſo as to prevent the paſſage of the urine, and thereby haſten the cloſing of the inciſion. To apply it properly in theſe caſes, I paſſed a collar faſtened to the neck of the patient, through the ring F, in the upper ſcrew worm, and the bandage which ſupported the dreſſings, through the ring G, in the lower ſcrew worm; and I have frequently experienced the ſucceſs of this manœuvre.

In the year 1757, I made uſe of it to preſerve an arm, fractured near the ſhoulder-joint, in its natural ſtate of extenſion, by faſtening the two flat rings F G, in the two pieces of wood H I; placing H under the arm-pit, and I upon the fore-arm againſt the bend of the elbow; and keeping the fore-arm bent by the ſling K L.

I alſo applied this inſtrument in the preſent caſe; and in order to aſſiſt in giving the proper direction, and neceſſary ſolidity to the part, I ſupported the arm with a vambrace, or half-canal, made of one very thin piece of wood, which ſurrounded two thirds of the circumference of the limb. The whole

was



Drawn by Mrs. David Daughter of the Author

J. Mynde sc

was faſtened by the bandages commonly uſed in fractures.

On the 15th of May, the twenty-ninth day after the operation, the wound having filled up very faſt, the arm appeared to have a ſort of firmneſs, that the muſcles alone were incapable of giving it; and on the 30th of the ſame month it had acquired a degree of ſolidity, which was nearly equal to the hardneſs of bone; for it was ſtrong enough to ſupport itſelf, and yielded very little to the preſſure that was made upon it; but the patient was ſtill unable to make any uſe of it, till the fifty-fifth day after the operation, at which time he began to move it a little.

It was ſcarce poſſible that ſo remarkable a caſe ſhould not be attended with ſome accidents, in the courſe of the cure; ſo that, on the 16th of June, I was not at all ſurprized at the appearance of a puſtule on the upper part of the ſcar, from which there was an oozing of matter. On the next day I paſſed a probe into this opening, which entered with ſome difficulty the length of an inch; but I did not find any ſplinters of bone, which I expected to meet with.

On the 18th of June, I made a large opening in this part, and extracted a point of bone, which ſeemed to have ſhot out in a very particular manner towards this puſtule, and might probably have contributed, by its irritation, to have produced this fiſtula.

On the 19th of June, I took away the remains of this bony ſubſtance, ſituated underneath the above fiſtula, and which was but ſlightly attached to the neighbouring ſoft parts; after which the cure of this wound was compleated in fifteen days.

On the 25th of July, the hundredth day after the operation, I perceived another puſtule breaking out near the lower part of the ſcar. I then began to ſuſpect, as there had been a flow of matter from the arm for ſeveral years, that the conſtitution had acquired a habit of diſcharging itſelf at this part; and attributed to that cauſe theſe trifling relapſes, as well as the former more conſiderable one, which the patient had ſuffered.

From whatever ſpring theſe habitual diſcharges may be derived, and whatſoever may be the reaſons aſſigned for the relapſes they produce, it is well known that they are ſometimes diverted or cured by iſſues; I therefore ordered a large cauſtic to be applied to the patient's left leg. The arm was at this time very ſolid to the touch, but ſtill yielded a little to its own weight, as the branch of a tree, or a piece of green wood; I therefore had a bracelet of whalebone made to ſupport and keep it ſteady.

On the 1ſt of Auguſt, the puſtule, which I had perceived on the 25th of July, appeared to be much increaſed in ſize, and was ſpread out into fungous fleſh: I paſſed a probe into it, and found ſome pieces of bone bare towards it's upper part.

On the 10th of Auguſt, a ſmall ſcale was exfoliated, and from that time the fungous fleſh began to diſappear.

In September, five months after the operation, the iſſue afforded a very plentiful diſcharge, and had attracted the humour ſo powerfully towards this part, that the leg on which the cauſtic had been applied, appeared ſwelled and grew painful; but, at the ſame time, the

the arm was entirely cured; having recovered all its actions and uſes, together with its proper form and length. And on the 12th of October, Romain was diſcharged from the hoſpital, very thankful for the recovery of his health, and the perfect cure he had received. He then went to rejoin his company at Dieppe, and reſume his former employments.

This obſervation, at the ſame time that it furniſhes a remarkable inſtance of animal vegetation, ſtrongly encourages ſurgeons to attempt the preſervation of limbs, in all caſes, where there is a poſſibility of bringing about this ſort of regeneration, ſo uſeful to mankind, and ſo honourable to the art.

Received August 1, 1766.

XXXIV. *A Letter from Mr.* Wargentin, *F. R. S. and Secretary of the Royal Academy of Sciences at* Stockholm, *to the Rev. Mr.* Maskelyne, *M. A. F. R. S. and Astronomer Royal at* Greenwich, *containing an Essay of a new Method of determining the Longitude of Places, from Observations of the Eclipses of* Jupiter's *Satellites.*

Vir reverende atque celeberrime,

Read June 19, 1766.

ANTE triennium fere, dominus Ellicott tuo nomine ad me misit exemplar libri a te editi, *The British Mariner's Guide*, pro quo gratissimo munusculo maximas tibi ago gratias, debitasque statim persolvere voluissem, nisi eodem tempore indicasset Ellicottus noster, te tunc esse absentem, occepto videlicet longo in insulam Barbadoes itinere. Benigne excuses precor, quod jam demum officii memor, tibi gratuler felicem reditum, & munus astronomi regii nuper in te collatum, quo ut diu, in astronomiæ incrementum, fungaris, opto, & ut illustrissimorum prædecessorum tuorum æmulus, parem acquiras gloriam.

Dictus liber fuit mihi duplici nomine acceptissimus, & argumenti pertractati elegantiâ, & ut amicitiæ sponte

ſponte mihi a te oblatæ teſſera. Non honori tantum ſingulari, ſed et certiſſimo uſui mihi erit commercium litterarium cum primarii obſervatorii aſtronomo. Dabo operam, ut non prorſus indignus tuo favore videar.

Ne vacuæ ad te veniant primæ litteræ meæ, diſſertatiunculam aſtronomici argumenti inſeram, cum obſervationes alicujus momenti, quas tibi communicem, jam non habeam.

Quamvis non dubitem, quin jam diu determinaveris longitudinem geographicam inſulæ Barbatorum, ope obſervationum ibi a te habitarum, quæ extant in noviſſimo Tranſactionum Philoſophicarum tomo; non tamen ingratum tibi futurum ſperaverim meum videre tentamen in ea re, quod ſimul eſt ſpecimen methodi, quâ, nullibi quidem antea diſtincte explicatâ, per plures annos uſus ſum, quando ex obſervationibus ſatellitum Jovis, in aliquo diſſito loco inſtitutis, ejus loci longitudinem elicere conatus ſum.

Conſueta & diu uſitata methodus eſt, ut conferantur obſervationes habitæ in loco, cujus longitudo quæritur, cum obſervationibus inſtitutis eodem tempore in obſervatorio quodam vel alio loco, quo longitudo determinatur. Si quæ inter illas occurrunt immediate correſpondentes, ita ut eædem immerſiones vel emerſiones Satellitis cujuſdam, præcipue primi utrinque, notatæ ſint, quodvis par correſpondentium ſuppeditat differentiam quandam meridianorum; & ſi hoc modo inventæ plures differentiæ aliquantulum inter ſe diſcrepant, media inter omnes ſumta pro verâ vel veræ proximâ haberi ſolet. Satis bene ſuccedit hæc methodus, quando plurimæ ſuppetunt obſervationes correſpondentes tam immerſionum quam emerſionum

primi,

primi, habitæ tubis vel telescopiis fere æqualis utrinque potentiæ. Et si sequamur regulas, quas huic negotio præscripsit celeberrimus astronomus Cæsareus Vindebonensis, pater S. J. Maximilianus Hell, in ephemeridibus suis astronomicis ad annum 1765, quæsitam meridianorum differentiam acû fere, quod aiunt, tangere licet, ne obstante quidem instrumentorum discrepantiâ & observatorum inæquali perspicaciâ. Laborat tamen illa methodus imperfectione quadam. Plerumque multæ in loco determinando factæ observationes nullam nactæ sunt in alio loco correspondentem; tum vero omnes illæ nullius fere in geographiâ sunt usus & quasi perditæ, quamvis fortassis per se sint præstantissimæ. Quid? si una tantum alterave, vel nulla prorsus reperiatur correspondens: contingit autem hoc sæpissime, imprimis si locus, cujus longitudo quæritur, longe remotus est versus orientem vel occidentem ab Europâ, astronomiæ sede. Sic parti longe maximæ observationum Pekinensium deficiunt correspondentes Europeæ. Hujus quoque rei exemplo esse possunt tuæ, in insula Barbatorum versus finem anni 1763, & initio 1764 habitæ: nam inter 17 a te observatas eclipses primi satellitis, non nisi duæ, quantum quidem mihi constat, correspondentes habuerunt.

Ad supplendum correspondentium defectum, astronomi duplici incedere consueverunt viâ. Alii quærunt, anne aliqua eclipsium satellitis proximæ præcedentium, vel sequentium in observatorio quodam notata sit, quâ inventâ, & addito vel subtracto revolutionum intercedentium noto numero, invenire satagunt verum tempus, quo contingere debuisset in eodem observatorio eclipsis desiderata correspondens.

Sed

Sed aleæ plena est hæc methodus, revolutiones enim satellitis inæquales sunt, ut vix duæ vicinæ perfecte conveniant. Inæqualitates plerasque eruere docet quidem theoria, sed operosus labor est & lapsui obnoxius. Nonnunquam deficiunt observationes etiam hoc modo, mediate scilicet, correspondentes, vel nimis longe sunt remotæ a tempore desiderato, vel si quæ occurrit propinquior, una pluribus non sufficit. Sic toto mense Decembri, quo tu in dictâ insulâ septem observasti emersiones primi satellitis, unica tantum, quod sciam, in Europâ videri contigit. Si unica ista minus bona fuisset, & tamen omnes tuas ad illam exigere oporteret, anne in erroneam induceret meridianorum differentiam, tuis quantumvis per se optimis?

Melior sane est altera ratio supplendi defectum correspondentium, ope calculi ad datum meridianum facti, & addito vel subtracto tabularum errore, correcti, quem errorem aliæ observationes, eodem mense vel proximis habitæ, prodiderunt: nam hoc modo omnes observationes, in loco determinando institutæ, pro ratâ parte contribuunt ad indagandam longitudinem. Certitudo hujus methodi, quam ego potissimum expoliendam reor, ab eo pendet, ut pro quovis momento rite corrigatur error, cui tabulæ, etiam optimæ, sunt obnoxiæ. Facile hoc fieret, si error iisdem annis vel saltem mensibus constans esset. Sed experientia docuit, observationes, circa idem tempus factas, quamvis ipsis observatoribus fere æqualiter bonæ sint visæ, inæqualiter tamen a calculo recedere; immo eandem immersionem, vel emersionem, a diversis astronomis notatam, sæpe multis secundis quin & integro minuto differentem dare calculi errorem, non totum tabulis vitio vertendum; sed quâ partem

 ipsis

ipsis observationibus. In hoc casu, valde frequenti, arduum est invenire verum tabularum errorem, nam, insciis observatoribus etiam exercitatissimis, sæpissime irrepunt defectus, ut pro certis venditent observationes vitii non expertes. Si itaque, ut plerumque fit, una tantum alterave seligatur observatio pro inveniendo tabularum errore, facile in devia itur.

Hisce rationibus persuasus, necessarium omnino duco examinare ipsas observationes, antequam adhibeantur determinandæ per quamcunque methodum longitudini. Fieri id potest modo sequenti.

Omnes, quæ suppetunt, observationes ejusdem satellitis, præcipue primi, habitas circa idem tempus, tam in loco determinando quam in observatoriis satis antea determinatis, dispono, secundum temporis seriem, in tabulam. Differentiam meridianorum quæsitam tantisper assumo, quantam vel observationes, si quæ adsunt, immediatè correspondentes offerunt, vel calculus non correctus unius observationis requirere videtur. Deinde ad tabularum amussin exigo omnes observationes, & inventum calculi errorem juxta quamvis annoto. Quo facto, & attentè examinatâ errorum serie, satis patet, 1°, quænam inter observationes in locis determinatis habitas reliquis sint meliores & fide dignissimæ: 2°, quis eo tempore medius sit tabularum error; & denique, 3°, an quæsita meridianorum differentia major vel minor sit, quam quæ in calculo assumta erat, quantumque vel augenda vel minuenda erit, ut quam proxime pares prodeant calculi errores.

Quod ad primum attinet, verisimillimum utique puto, immersiones, quæ plerisque aliis multo citius, & emersiones quæ serius visæ sunt, inter minus certas

esse

esse referendas, etiamsi bonas crediderint ipsi observatores: nam theoria satellitum Jovis, imprimis duorum inferiorum, hoc tempore adeo exculta est, ut vix dubitare liceat, quin omnes ejusdem anni vel saltem mensis immersiones fere æqualiter a calculo recederent, si observationes æquali gauderent certitudine. Idem de emersionibus seorsim dicendum. Ideoque variationes &c, ut ita dicam, saltus errorum, inæquali observationum bonitati videntur tribuendæ. His aliisque circumstantiis, præsertim observatorum de quavis observatione annotationibus bene pensitatis, sat in propatulo erit verus vel medius tabularum eo tempore error. Si observationes in loco determinando habitæ pariter fere a tabulis discrepant, indicio est, assumtam tantisper meridianorum differentiam esse veram: sin minus, quantum augenda, vel minuenda sit. Si immersiones majorem minoremve poscere videntur meridianorum differentiam, quam emersiones, id inæquali tuborum potentiæ adscribendum, & media inter utrasque meridianorum differentia tanto certius pro verâ haberi potest, quanto plures utrinque observationes eam confirment.

Sit exempli loco determinanda longitudo insulæ Barbatorum, ope 17 observationum primi satellitis ibi a te habitarum, mensibus Novembri et Decembri 1763, Januario et Februario anni 1764. Reperio alias 21 observationes iisdem mensibus factas in Europâ, in locis probe determinatis, quarum duæ tantum tuis sunt correspondentes, quæ per medium dant differentiam 5 horarum, 10 minutorum, & 14 secundorum, inter meridianum observatorii Stockholmensis tuique loci. Scire lubet, an illa reliquis quoque observationibus congruat. Omnes itaque horum mensium observationes, additis ultimis in Octobri 1763, & primis in Martio 1764, in justum ordinem redigo, &

verum tempus ſecundum tabulas computo, quo quivis in ſuo meridiano contingere debuiſſet.

Peracto calculo, apparet, immerſiones Europæas alias paucis ſecundis ſerius, alias citius obſervatas fuiſſe, quam per tabulas oportuerat. Illas meliores jure puto, has, præcipuè quæ maxime diſcedunt, inter dubias refero. Per medium, immerſiones omnes calculo quam proxime convenire debuiſſe videntur. At inter 15 in Europâ obſervatas emerſiones, pleræque calculum aliquot, ubi maxime, 31 ſecundis præverterunt, exceptis tribus Tyrnavenſibus, quæ computatis momentis ſerius viſæ, ideoque vitio non carere cenſendæ ſunt. Media bonarum emerſionum præceſſio fuit 20 ſecundorum circiter.

Conſiderato ulterius tuarum obſervationum calculo, vidi eas fere omnes, poſitâ differentiâ meridianorum $5^h\ 10'\ 14''$, multo magis prævertere computum, quam Europæ, ſed fere æqualiter a tabulis diſceſſuras, ſi differentiam meridianorum 22 ſecundis auxerim. Eam itaque $5^h\ 10'\ 36''$, quam proxime æqualem autumo; cujus rei ut fidem tibi faciam, heic ſubjungo omnes obſervationes, cum correcto tuarum calculo.

Pariſienſes habitæ ſunt teleſcopio Gregoriano, diametros objectorum 104^{ies} augente, à Domino Meſſier, in Obſervatorio Societatis Maritimæ, quod duobus temporis ſecundis orientalius eſt obſervatorio regio.

Viennenſes, à Rev. Patre Hell, S. J. teleſcopio $4\frac{1}{2}$ pedum, in obſervatorio Cæſareo, cujus à Pariſienſi longitudo præciſe eſt ſtabilita $56'\ 11''$.

Tyrnavenſes a R. P. Weiſs, S. J. teleſcopio 4 pedum Newtoniano. Differentia meridiani Tyrnaviæ à Pariſienſi obſervatorio eſt $1^h\ 1'\ 56''$ ſatis certa.

Meæ habitæ ſunt tubo Dollondiano 10 pedum egregio. Diſtat autem obſervatorium Stockholmenſe à Pariſienſi $1^h\ 2'\ 51''$, & à Grenovicenſi $1^h\ 12'\ 7''$.

OBSERVATIONES COMPARATÆ PRIMI SAT. JOVIS.

Ann.	Menf.	Tempus Obfervat. D.	h	′	″		Tem. comput. h	′	″	Error comp. ′	″	Locus Obf. cum Annotationibus.
1763.	Oct.	23	13	32	21	Imm.	13	32	9	0	12	— *Paris.* Serenum.
		23	14	27	50		14	28	18	0	28	+ *Vien.* Cœlo vaporofo.
		25	8	56	26		8	56	56	0	30	+ *Vien.* Ser. fed lunâ vicinâ.
		25	9	3	48		9	3	36	0	12	— *Stockholm.* Bona vifa, cœlo fer.
	Nov.	1	9	55	6		9	54	59	0	7	— *Paris.* Serenum.
		6	18	21	14		18	21	10	0	4	— *Tyrnav.* Cœlo fereno.
		8	12	44	24		12	44	49	0	25	+ *Vien.* Cœlo vaporofo.
		10	7	17	36		7	17	48	0	12	+ *Tyrnav.* Cœlo fereno.
		13	15	5	38	Imm.	15	5	37	0	1	— Infula *Barbad.*
		15	14	37	34		14	37	44	0	10	+ *Vien.* Cœlo fudo.
		17	8	9	39		8	9	47	0	8	+ *Paris.* Obfervatio bona.
		20	16	57	20		16	58	10	0	50	+ Inf. *Barb.* Per tenues nubes.
		22	11	26	6		11	26	12	0	6	+ Inf. *Barb.*
		22	16	34	10		16	34	53	0	43	+ *Tyrnav.* Serenum.
		24	10	2	13		10	2	0	0	13	— *Paris.* Serenum.
	Dec.	1	7	46	0	Imm.	7	45	58	0	2	— Inf. *Barb.* Sat. contiguus Jovi.
		6	17	21	4	Em.	17	18	44	2	20	— Inf. *Barb.* Jupiter imminet horizonti.
		8	11	46	35		11	46	33	0	2	— Inf. *Barb.*
		10	11	24	45		11	24	56	0	11	+ *Stockholm.* Serenum.
		15	13	37	9		13	37	41	0	32	+ Inf. *Barb.*
		17	8	5	46		8	5	27	0	19	— Inf. *Barb.* Aëre turbido.
			15	28	35		15	28	49	0	14	+ Inf. *Barb.*
		24	9	56	20		9	56	33	0	13	+ Inf. *Barb.*
64	Jan.	2	11	25	59		11	26	17	0	18	+ *Stockholm.* Cœlo ferenifiimo.
		2	11	19	10		11	19	37	0	27	+ *Vien.*
		4	5	47	15		5	47	29	0	14	+ *Vien.*
		7	13	39	43		13	39	30	0	13	— Inf *Barb.* Aëre turbido.
		9	8	7	20		8	7	29	0	9	+ Inf. *Barb.*
		16	9	59	38		9	59	52	0	14	+ Inf. *Barb.*
		23	11	32	36		11	52	52	0	16	+ Inf. *Barb.*
		25	6	20	51		6	21	15	0	24	+ Inf. *Barb.* Optima.
		25	11	31	20		11	31	51	0	31	+ *Stockholm.* Certa.
	Febr.	8	10	9	19		10	9	50	0	31	+ Inf. *Barb.*
		10	8	46	6		8	46	24	0	18	+ *Paris.* Bona.
		10	9	47	41		9	47	18	0	23	— *Tyrnav.*
		17	10	41	48		10	41	51	0	3	+ *Paris.* Bona.
		17	11	43	6		11	42	45	0	21	— *Tyrnav.*
		17	11	44	22		11	44	40	0	18	+ *Stockholm.* Cœlo fereno, fed vehementiori vento.
		19	6	13	22		6	13	39	0	17	+ *Stockholm.* Certe jam aderat; forte jam ante cœperat.
		19	6	6	34		6	6	59	0	25	+ *Vien.*
		19	6	11	45		6	11	44	0	1	— *Tyrnav.*
		24	8	30	42	Em.	8	30	13	0	29	— Inf. *Barb.*
	Mart.	4	10	4	51		10	4	53	0	2	+ *Tyrnav.*
		4	10	6	41		~~10~~	~~6~~	~~8~~	~~0~~	~~7~~	~~+ *Stockholm.*~~ Aurora Borealis circa Jovem.
		13	6	26	48		6	26	34	0	14	— *Vien.*
		13	6	31	45		6	32	19	0	34	— *Tyrnav.*

Jam ipſi tibi, vir celeberrime, relinquo dijudicandum, anne ſenſibiliter majorem vel minorem admittant hæ obſervationes meridianorum differentiam? Tuarumne idem fere inter ſe & cum tabulis conſenſus, atque eſt Europæarum? Videſne, quaſi ictu oculi, quæ obſervationes erroris ſunt ſuſpectæ, quæ reliquis præferendæ? Quas ipſe dubias notaſti, calculus quoque arguit. Emerſio die 6 Dec. omnium maxime, ſine dubio, eſt vitioſa. Inæqualem tuborum poteſtatem etiam indicat errorum ſeries: nam Weiſſius ſuo fere ſemper obſervavit immerſiones citius, & emerſiones tardius, quam reliqui. Tuum teleſcopium meo Dollondiano fere palmam præripere videtur. Si differentia meridianorum inventa non eſt exacte vera, eam potius aliquot ſecundis augendam quam minuendam puto, ſed paucis.

Pari ratione differentiam meridianorum obſervatorii Grenovicenſis & inſulæ S. Helenæ inveni 23 minutis primis vix majorem, ſed potius aliquot ſecundis minorem, niſi duæ ex emerſionibus 15 ibi a te obſervatis ſint prorſus rejiciendæ: immerſiones enim ſolæ paulo majorem admitterent.

Perſuaſus ſum, hanc methodum determinandi longitudines per obſervationes ſatellitum Jovis eſſe reliquis præferendam, quod evidentior & generalior ſit, & præcipue quoniam ſimul indicat obſervationum majorem minoremve præſtantiam. Sed ſatis de his.

Vale, vir æſtumatiſſime, & fave,

Reverendi celeberrimique nominis tui,

Cultori ſincero,

Dab. Stockholmiæ,
die Julii, 1766.

Petro Wargentin.

XXXV. *A Letter from* John Ellis, *Esq*; *F. R. S. to the President, on the* Coluber Ceraftes, *or* Horned Viper *of* Egypt.

My Lord,

Read Dec. 11. 1766.

THE Coluber ceraftes, or horned viper of Egypt, which I have the honour to prefent a fpecimen of to this illuftrious Society, I am informed, is very rare, and fcarce to be found in any of the cabinets of natural curiofities in Europe. Befides, the authors who have treated on the Ceraftes, as Alpinus and Bellonius, have given fuch unfatisfactory defcriptions of it, and inaccurate figures, that I thought an exact drawing from nature, together with the beft and lateft fyftematical account of it, would be agreeable, as well to the lovers of antiquity as natural hiftory.

The ancient Egyptians moft certainly efteemed it a hieroglyphic of fome importance; for when we examine their monuments of the greateft antiquity, fuch as their obelifks, temples, ftatues, palaces, and even their mummies, we are almoft fure to find many reprefentations of it on them. Thofe two immenfely large ftones, lately brought from Alexandria, in Egypt, now in the court-yard of the Britifh Mufeum, which appear to be part of the grand cornice of fome magnificent palace, have many figures of the Ceraftes curioufly engraved upon them.

Dr.

Dr. Haſſelquiſt, a pupil of the celebrated Linnæus, who was in Egypt in 1750, has given us a particular deſcription of this curious animal; but neither he nor the former writers on Egypt, that mention the Ceraſtes, ſay any thing about the venom of its bite. This we are informed of only by Dr. Turnbull, who lived many years in Egypt, both at Alexandria and Cairo, and who was ſo kind to preſent me with two ſpecimens of it.

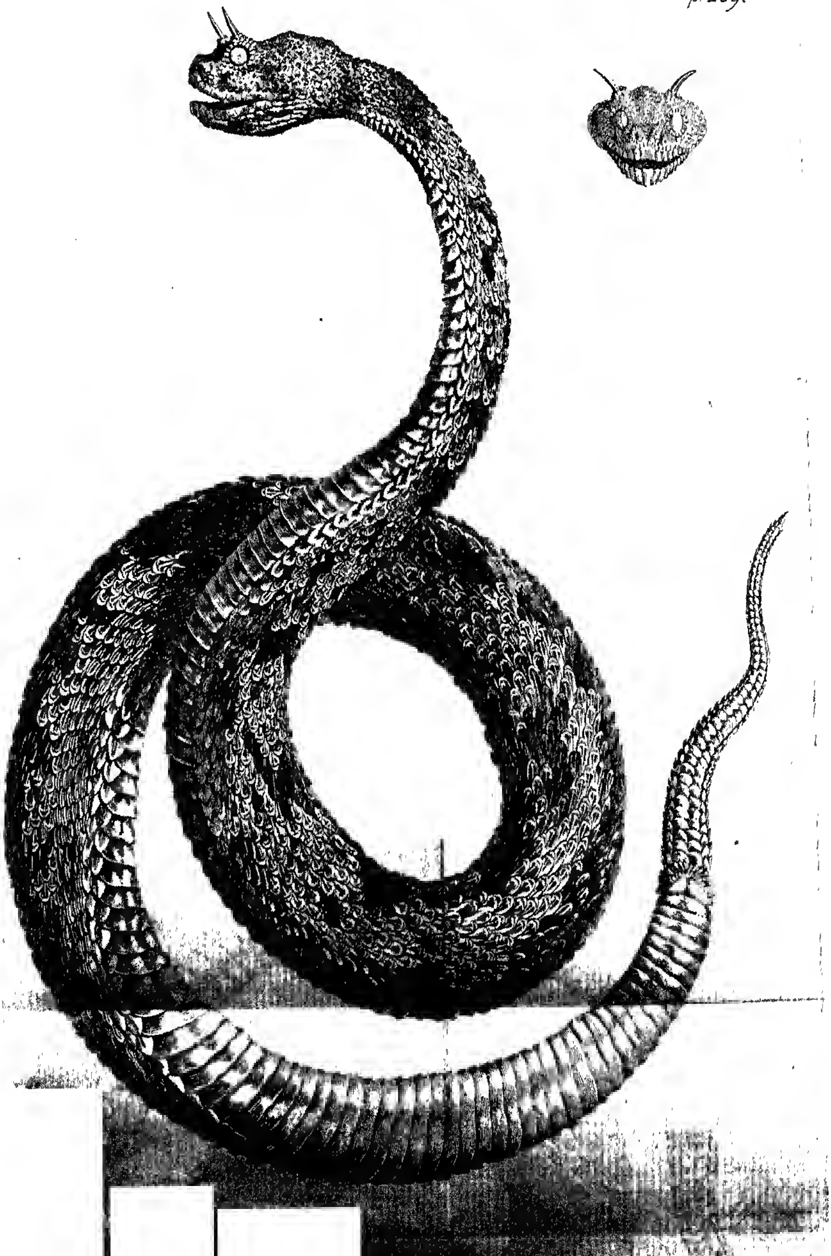
Dr. Linnæus, in his Syſtem of Nature, p. 217, calls it Coluber ceraſtes.

Dr. Haſſelquiſt, in his Iter. p. 315, Coluber cornutus; the following is an extract from his deſcription.

The head,	between the horns, is much depreſſed; the cheeks are ſwelled out, ſo that the hinder part of the head is conſiderably thicker than the neck; the ſnout is ſhort and blunt; the outward front of the upper and under jaws have a ſmall cavity, or depreſſion, in both; the noſtrils project like thoſe of a pug dog.
The eyes	have a perpendicular narrow and black pupil; the iris is of a yellowiſh grey colour; the orbits of the eyes are neatly ſet round with ſmall hemiſpherical ſcales.
The tongue	is divided at the extremity into two parts.

The

COLUBER *Cerastes* Linnæi. *Philos. Trans. Vol. LVI. TAB. XIV.*
p. 289.



The teeth. In the upper jaw there are no teeth, but two bones placed lengthways in the palate; in them are fixed ſeveral ſmall teeth, generally about ten: they ſharp, of an equal length, and bend a little towards the throat. On the ſides of the under jaw, near the ſnout, are placed three or four teeth; but none quite in the fore part or hinder part.

The horns. Juſt above the eyes, near the upper part of their orbit, are two tentacula, which we call horns, about a quarter of an inch long; they are not ſtraight, but bend a little outwards; they are channelled lengthways, ſharp-pointed, but not very hard; their baſis is ſurrounded with a circle of ſmall erect ſcales.

The body is narrow towards the neck; the diameter of the thickeſt part of the middle about one inch; the tail grows ſuddenly taper, and ends in a ſharp point.

The colour. The top of the head, the back, and upper part of the tail, are variegated with large irregular ſpots, of a bright ochry colour, or reddiſh brown; the throat, belly, and under part of the tail, are whitiſh.

The length of this ſpecimen (ſee TAB. XIV.) is as follows; from the noſe to the anus 22 $\frac{1}{2}$ inches,

inches, the tail 3 $\frac{1}{2}$ inches; ſo that the whole ſerpent is 26 inches long.

The Belly is covered with 145 broad ſcales, or ſcuta; the tail with 43 pair of ſmall ſcales, or ſquamæ.

The number of ſquamæ and ſcuta have been thought by late authors to be the beſt method of determining the ſpecies of ſerpents; but they are not ignorant that they differ a few now and then: Haſſelquiſt reckoning 150 ſcuta, and 50 pair of ſquamæ, to his Coluber cornutus.

I am, my Lord,

your Lordſhip's

moſt obedient humble ſervant,

John Ellis.

To the Right Hon. the Earl of Morton,
Preſident of the Royal Society.

XXXVI. *Ab-*

XXXVI. *Abſtract of a Journal of the Weather in* Quebec, *between the 1ſt of* April 1765, *and 30th of* April 1766. *By Cap.* Alex. Roſe, *of the 52d Regiment*; *communicated by the Rev.* P. Murdoch, *D. D. F. R. S.*

Read December 18, 1766.

Days		Heighth on Fahrenheit's Scale. Greateſt	Leaſt	Winds	Weather
1765					
Apr.	1	34½	30	E.	Sunſhiny morning; afternoon overcaſt; night ſnow
		53	31	N.W.	Calm ſunſhiny day; at night froſt
	10	39	30	E.	Fine ſunſhiny day, briſk wind
	15	60	31	N.W.	Sunſhiny day; evening rain
	20	41	38	N.W.	High wind the whole day; night hard froſt
	25	47	39	E.	Ditto
	27	44	38	E.	Ditto
	30	60	49	E.	Ditto
May	1	56	45	S. E.	Overcaſt morning; afternoon heavy rain
	5	59	47	E.	Sunſhiny day, briſk wind
	10	54	37	N.	Ditto ditto
	12	73	43	W.	Ditto ditto
	15	44½	39½	E.	Overcaſt the whole day, briſk wind
	20	56	41	S. E.	High wind with rain
	27	85	58	W.	Sunſhiny day, morning calm, afternoon high wind
	31	67	46	W.	Fine ſunſhiny day, little wind.

Days		Heighth on Fahrenheit's Scale. Greatest	Least	Winds	Weather
1765					
June	1	67	53½	W.	Fine sunshiny day; afternoon E.
	3	80	63	W	Sunshiny morning; afternoon cloudy with rain
	5	69	51	E.	Sunshiny day, little wind
	9	87	64	S.	Sunshiny day; afternoon W.
	15	52	45	N.W.	Cloudy day, brisk wind
	19	69	54	E.	Sunshiny day; night rain
	25	62	47	N.W.	Cloudy day, brisk wind
	27	68	50	N.W.	Fine calm sunshiny day; night rain
	29	74	61	W.	Ditto ditto
	30	62	60	E.	Sunshiny day, brisk wind ditto
July	1	60	55	E.	Rain the whole day
	5	72	58	W.	Morning sunshiny; afternoon thunder with rain
	10	80	62	S. W.	Sunshiny day
	15	73	58	E.	Ditto
	19	84	65	W.	Ditto
	23	77	60	E.	Ditto
	28	84	76	N.	Morning sunshiny; afternoon high wind with thunder
	30	71	56	W.	Brisk wind with flying clouds
Aug.	1	69	53	N. E.	Ditto ditto; aftern. S. W.
	5	76	57	W.	Sunshiny morning; afternoon showery
	9	79	64	S. W.	Ditto
	14	62	50	N.W.	Sunshiny day, brisk wind
	17	66	47	W.	Overcast the whole day; night much rain
	21	67	51	W.	Fine sunshiny day
	28	74	68	E.	Ditto
	31	69	56	W.	Ditto

Days	Heighth on Fahrenheit's Scale. Greatest	Least	Winds	Weather
1765				
Sept. 1	69	52	W.	Ditto, brisk wind
3	70	55	E.	Ditto, calm
5	68	59	E.	Rain the whole day
12	50	40	N.W.	Sunshiny day, brisk wind
17	64	47	W.	Ditto, calm
26	40	33	N.W.	Overcast the whole day; afternoon E.
29	52	38	S. E.	Sunshiny day; afternoon S. W.
Oct. 1	56	47	S. W.	Sunshiny day
6	56	39	W.	Ditto
9	60	53	S. E.	Fogg with rain the whole day
11	38	34	W.	Overcast, with high wind; some snow for the first time
12	41	28	W.	Sunshiny calm day
20	55	54	E.	Rain with high wind the whole day
23	32	19	W.	Sunshiny calm day
29	40	36	W.	Overcast the whole day
31	35	34	E.	Heavy rain the whole day
Nov. 1	40	30	S. W.	Fine calm sunshiny day
5	31	26	W.	Cloudy with snow
11	40	37	W.	Foggy morning; afternoon sunshine
14	31	30	E.	Heavy storm of snow, from this day the ground remained covered
18	36	33	W.	Overcast the whole day
23	20½	12	W.	Sunshiny day, little wind
26	22	7	W.	Ditto; afternoon E.
30	31	30	W.	Overcast, some snow
Dec. 1	21	18	W.	Sunshiny day
6	13	2	S. W.	Ditto
9	29	29	E.	Overcast, little snow
14	25	4	W.	Sunshiny day
18	31	25	E.	Overcast, little snow
23	0	b 12	W.	Sunshiny day
24	7	b 11	W.	Ditto morning, snow afternoon

Days	Heighth on Fahrenheit's Scale. Greatest	Least	Winds	Weather
1765				
Dec. 25	29	0	W.	Overcaſt the whole day
30	^{b}17	^{b}24	W.	Sunſhiny day, briſk wind
31	^{b}15	^{b}29	W.	Ditto
1766				
Jan. 1	^{b}3	^{b}28	W.	Morning overcaſt, little wind; afternoon briſk wind E.
2	12	^{b}5	E.	High wind, little ſnow
3	10	^{b}14	W.	Briſk wind and cloudy
4	^{b}6	^{b}19	W.	Briſk wind, ſunſhiny day
5	25	^{b}25	W.	Overcaſt the whole day
10	25	19	W.	Ditto
12	0	^{b}5	W.	High wind, clear weather
14	29	9	E.	High wind, overcaſt
16	10	^{b}10	W.	Ditto ditto
17	^{b}2	^{b}17	W.	Ditto, clear weather
21	37	33$\frac{1}{2}$	S. E.	Foggy with rain the whole day (earthquake at 5 o'clock next morning)
23	^{b}10	^{b}23	N.W.	Briſk wind
24	2	^{b}25	W.	Sunſhiny day
25	22	10	E.	Morning ſnow; afternoon clear
31	27	4	S. W.	Sunſhiny day
Feb. 1	6	^{b}5	W.	Sunſhiny day, high wind
2	6	^{b}10	W.	Ditto, little wind
7	28	25	E.	Overcaſt, high wind
14	39	18	W.	Calm ſunſhiny day
20	6	^{b}6	W.	Sunſhiny day, briſk wind
23	26	16	E.	Heavy fall of ſnow, briſk wind
25	5	^{b}10	W.	Calm ſunſhiny day
28	35$\frac{1}{2}$	28	S. W.	Overcaſt the whole day, briſk wind
Mar. 4	19	3	S. W.	Sunſhiny day, high wind
7	36	26	S. E.	Briſk wind, with flying clouds

Days	Greatest } Height on Fahrenheit's Scale.	Least	Winds	Weather
Mar. 12	40	27	E.	Flying clouds, with ſnow
14	29	^{b}2	N.W.	High wind, with ſnow
15	2	^{b}10	N.W.	High wind the whole day
16	23½	^{b}5	S. E.	Fine calm ſunſhiny day
17	36	31	S. E.	Overcaſt and ſhowery the whole day
23	27	22	E.	Snow the whole day, briſk wind
27	32	9	W.	Sunſhiny day
31	46	29	N.	Ditto
Apr. 1	42	25	W.	Ditto, afternoon E.
3	46	19	W.	Ditto
10	42	36	E.	Ditto, little wind
13	48	35	W.	Ditto
19	42	19	W.	At night ſnow; the leaſt ſnow this ſeaſon
22	27	26	W.	Sunſhiny day
29	51	31	W.	Ditto
30	42	30	E.	Rain and overcaſt the whole day

N. B. The letter ^{b} placed over any figure, ſignifies that the Mercury was ſo many degrees below 0. Thus on the 31ſt of December 1765, you find ^{b}15, ^{b}29, that is, 15 and 29 below zero; or 47 and 61 below the freezing point. Again, on the 5th of January 1766, the numbers are 25 and ^{b}25, which is 25 above, and 25 below 0. So that the difference between the higheſt and loweſt degrees of heat on that day, is 50 degrees.

On the 27th of May (1765), when the Mercury roſe to 85, and fell no lower than 58 (the mean of which is 71½) at Quebec, at a place down the river, called Mont Louis, was a heavy ſtorm of ſnow, which continued the whole day: the diſtance of theſe places is about 260 miles.

Alexander Roſe,

Lieutenant in the 51ſt Regiment.

XXXVII. *De-*

Received July 4, 1766.

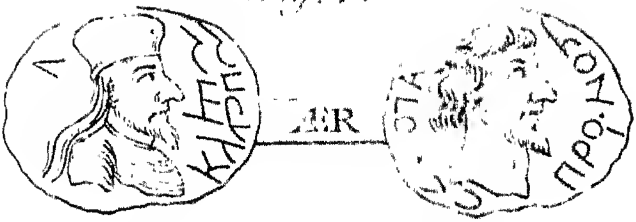
XXXVII. *Deſcription of two* Parthian *Coins, never hitherto publiſhed. By the Rev.* John Swinton, *B. D. F. R. S. Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany, *in a Letter to* Charles Morton, *M. D. Sec. R. S.*

Dear Sir,

Read Dec. 18, 1766.

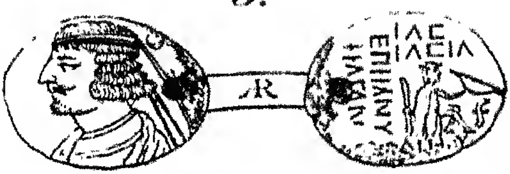
TOWARDS the cloſe of laſt month, a few days after my viſit to you at Montague-Houſe, I met with two antient braſs coins, pretty well preſerved, that had formerly had a place aſſigned them in the valuable collection brought by the Right Reverend Dr. Pococke, Lord Biſhop of Meath, out of the Eaſt. The workmanſhip of theſe pieces ſeems conſiderably different from that both of the Parthian coins, hitherto publiſhed, and thoſe ſtruck by the Perſian princes of the houſe of Saſſan. It ſomewhat, however, reſembles that of the braſs medals of one or two of the later Parthian kings. As I take them never to have appeared before in this part of the world, and highly to merit the attention of the curious; I ſhall beg leave, without any farther apology, to tranſmit accurate draughts of them to the Royal Society, together with a few curſory obſervations upon them, which may, perhaps, prove not altogether unacceptable

Fig. 1.



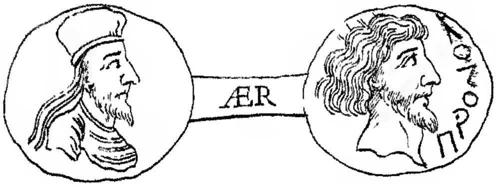
Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.

3.



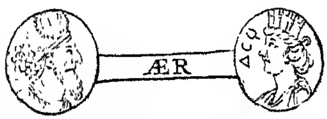
Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.

2.



Apud Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

4.



Penes Joannem Swinton, S.T.B. Oxonienſ. R.S.S.

unacceptable to that moſt illuſtrious and very learned body.

At firſt, from ſeveral ſlight circumſtances, I attributed theſe coins to one of the Perſian monarchs of the houſe of Saſſan; but examining them afterwards, with greater attention, I was rather inclined to believe, that they appertained to one of the latter Parthian kings. This, upon a farther inquiry, may perhaps appear probable, from the following conſiderations.

1. The workmanſhip of theſe pieces, however rude and inelegant, ſeems tolerably well to agree with that of ſeveral braſs medals of the later Parthian kings; whereas it almoſt totally differs from that of the coins generally aſſigned the Perſian monarchs, poſterior to thoſe kings.

2. The tiara of the prince exhibited by the medals before me is ſomewhat ſimilar to thoſe handed down to us by two Parthian coins, attributed, by Dr. Vaillant, to Vologeſes II. and Artabanus IV.

3. Theſe medals may naturally enough be ſuppoſed to have firſt appeared in ſome neighbouring province, reduced by the Parthian arms, rather than in the interior part of Parthia itſelf; which may poſſibly, in ſome meaſure at leaſt, account for the rudeneſs and inelegance of taſte they apparently preſent to our view.

4. The coins in queſtion ſeem to have had ſome Greek characters originally impreſſed upon them. If this be admitted by the learned, thoſe letters may poſſibly be thought to point at a Parthian mint. For we have ſcarce ever hitherto met with any Greek

characters on the medals (1) generally aſcribed to the Saſſanian kings.

5. The pieces under conſideration here have likewiſe ſeveral unknown characters upon them, which can by no means be deemed the ſame with thoſe preſerved on the Perſian coins ſtruck by the princes of the houſe of Saſſan.

Theſe two medals are of the ſize of the ſmaller middle Roman braſs, or nearly ſo. Their workmanſhip, as has been juſt remarked, is inelegant, or rather ſomewhat rude. They are ſo ſimilar to each other, that they may be conſidered, without any great impropriety, as duplicates of the ſame medal. Both of them, on the anterior part, ſeem to have retained the effigies of the ſame Parthian king; and, on the reverſe, they both exhibit a human head, with the hair formed into curls, on which is juſt viſible a rude ſort of crown. Before the face of the latter, the Greek elements Π, Ρ, Ο, Ζ, Ο, Υ, or ΠΡΟΖΟΥ, on both plainly enough appear; though one of them only preſents to our view, before the face of the Parthian king, a complex

(1) We meet, however, with a Greek date on a little braſs coin, very ill preſerved, in my ſmall collection, which ſeems to indicate the piece to have been ſtruck by one of the princes of the houſe of Saſſan. That date was undoubtedly formed of the letters ΔΟΦ, ſo that the firſt appearance of the medal muſt be allowed poſterior to the diſſolution of the Parthian empire, whether we adopt F. Corſini's Parthian Æra, or that followed by Dr. Vaillant. The coiffure of the prince's head is exactly the ſame with that peculiar to the Saſſanian kings. The globe alſo, above the diadem, and the large tuft of hair here, ſeemingly adorned with pearls or other precious ſtones, and forming another kind of globe, are frequently viſible on the medals appertaining to thoſe kings. The turrited head, on the reverſe, repreſents the genius of the city where the coin was ſtruck. *Philoſ. Tranſact.* Vol. LVI. TAB. XV. N. 4.

character, or monogram, seemingly composed of the Greek letters E,Λ, and three or four unknown characters, that have suffered a little from the injuries of time. Such are the coins now described, and the proper objects of my attention here.

With regard to the word, or rather name, ΠΡΟΖΟΥ, I shall not hesitate a moment to read and pronounce it ΠΕΡΟΖΟΥ; instances of the omission of a Greek letter, according to the (2) writers here referred to, having been met with on the Parthian coins. Nay, I find in my small cabinet an inedited Parthian coin, with the following imperfect inscription upon it, ΒΑΣ ⁞ ⁞ ⁞ ⁞ ΒΑΣΙΛ ⁞ ⁞ ⁞ ⁞ ΔΙ ⁞ ⁞ ⁞ ⁞ ΕΠΙΦΑΝΥ ⁞ ⁞ ΦΙΛΕΛΝ ⁞ ⁞ ⁞ ⁞ for ΦΙΛΕΛΛΗΝ ⁞ ⁞ to omit others that might, with equal facility, be produced. Nor can this be matter of surprize to any one who considers, that Greek words are sometimes very inaccurately expressed on those coins. The unusual curls, on the reverse, may possibly be thought to point at Armenia (3), as the country where these pieces were struck; especially, as the complex character, if it is a monogram formed of the Greek elements E,Λ, or EΛ, seems to direct us to the city of Elegia in Armenia, where a whole Roman army (4) was cut off by Vologeses II. And this will appear

(2) Edvard. Corsin. *Ad Clariss. Vir. Paul. M. Paciaud. Epist.* Romæ, 1757. Erasm. Froel. *Reg. Vet. Numism. &c.* Tab. I.N.X. Viennæ Austriæ, 1753. Vid. etiam *Saggi di Dissertazion. Accademich. &c. di Corton.* Tom. VII. p. 203. In Roma, 1758. *Nov. Act. Eruditor. Lips. Mens. August.* 1758. p. 436. Lipsiæ, 1758. & *Philos. Trans.* Vol. LVI. Tab. XV. N. 3.

(3) *Philosoph. Transact.* Vol. L. Par. I. p. 186.

(4) Jul. Capitolin. in *M. Antonin. Philosoph. Lucian.* in *Vit. Alexand. Pseudomant.* p. 486, 487.

ſtill more probable, after we have diſcovered the monarch denominated Perozes, or Peroz, and the reaſon of that name. To what prince therefore, and on what occaſion, the name, or rather ſurname, Perozes was applied, I am next to inquire.

Vologeſes II. having finiſhed his preparations for a war with the Romans, in the reign of Antoninus Pius, ſoon after that prince's death, made an irruption (5) into the Greater Armenia. This happened, according to Dio, in the year of Rome 915, or of CHRIST 161. Meeting with little or no oppoſition, he advanced to Elegia, a city of that kingdom, where a Roman army, under the command of Severianus, the prefect of Cappadocia, was at that time poſted. This formidable body he immediately attacked, (6) and ſo intirely defeated it, that ſcarce a ſingle Roman found means to eſcape. So complete a victory as this muſt of courſe have put Vologeſes in poſſeſſion of the greateſt part of Armenia, and particularly of the city of Elegia. After ſo important a conqueſt, the Parthian monarch may naturally enough be ſuppoſed to have cauſed the medals I am conſidering to have been ſtruck, and that in the town of Elegia. And that this was really the caſe, ſome will perhaps allow deducible from the monogram preſented to our view on one of theſe coins. Nay, that he derived the name, or ſurname, Peroz, or Perozes, itſelf from a ſucceſsful expedition he undertook againſt the Romans, we learn from Moſes Chorenenſis, the Armenian hiſtorian. " At which time, ſays this " writer (7), after the death of Titus the Second,

(5) Jul. Capitolin. & Lucian, ubi ſup.
(6) Dio, lib. LXXI. p. 802. B.
(7) Moſ. Chorenenſ. *Hiſtor. Armeniac.* lib. II. cap. LXI. p. 181. Lond. 1736.

king

" king of the Romans, called Antoninus Auguſtus,
" Peroz, or Perozes, king of the Perſians, (Parthians)
" made an irruption into the Roman empire; from
" whence he deduced the name Perozes, which
" ſignifies *The Conqueror*, or *The Victor*, having
" before been denominated Vologeſus, according to
" the Greeks, but by what name he went amongſt
" the Perſians I have not yet been able to learn."
Which paſſage ſeems not only to point at the defeat of Lucius Attidius in (8) Syria, but likewiſe at the terrible overthrow given the Romans in Armenia, ſoon after Marcus Aurelius and Lucius Verus aſcended the imperial throne.

I ſhall only beg leave at preſent to add, that the (9) Arabic Firûz and the Perſic or Armenian Peroz agree intirely in ſignification, (10) if they may not be conſidered as abſolutely the ſame word; that a Perſian king, named Firûz by the Arabs, (11) is called Perozes (Περόζης) by Agathias; that (12) Moſes Chorenenſis and one, at leaſt, of the medals here deſcribed mutually ſtrengthen and ſupport each other; and that I am, with all poſſible conſideration and eſteem,

S I R,

Your moſt obedient humble Servant,

Chriſt-Church, Oxon.
June 30, 1766.

John Swinton.

(8) Jul. Capitolin. ubi ſup.

(9) Al Makîn, Greg. Abu'l Faraj, Iſm. Abulfed, aliique ſcriptor. Arab. Firûz, (فيروز) is rendered *Felicitas*, *Victoria*, *&c.* by Golius; and by Meninſki *Victorioſus*, *Felix*, *Proſper.* It is a Perſic word.

(10) Golius, Meninſki, aliique lexicograph. Arab. & Perſic.

(11) Agath. Scholaſt. *De Imper. & Reb. Geſt. Imperat. Juſtinian.* Lib. IV. p. 137. Pariſiis, 1660.

(12) Moſes Chorenenſ. ubi ſup.

Erratum, in *Philoſ. Tranſact.* Vol. LIV.
Page *139. *note*, l. 2. *for* שע *read* שח.

XXXVIII. *An*

Received October 24, 1766.

XXXVIII. *An Account of a successful Operation for the Hydrops Pectoris, by* William Moreland, *Surgeon at* Greenwich; *communicated by* W. Watson, *M. D. F. R. S.*

Read Dec. 18, 1766.

AS very few instances are to be met with in medical or chirurgical writers, of the succefsful opening of the thorax in the dropsy of the breast; the following person's case, who was preserved by it in the most imminent danger of death, may encourage others under similar circumstances to perform the operation, which has hitherto been very rarely attempted.

Anne Harmsworth of Crooms Hill, Greenwich, of a thin, hectic habit of body, and subject to defluxions on the breast, about the latter end of the year 1760, complained of a smart, shooting pain in her right side, which somewhat affected her breast. Her evacuations by stool and urine were by no means deficient, nor was there any remarkable appearance on the part affected. A blister was applied, and oily medicines given, which relieved her in a few days, yet not so intirely but that she had returns of the pain at different times, though not sufficient to make her apply for advice, till November 1762, when she was seized with a much greater degree of the same kind of pain, attended with difficulty of respiration, a sense of weight on

on the diaphragm, and a quick pulſe, with a little more heat than uſual.

On the 18th of December, I ſaw her, for the firſt time, with Mr. Mills, a ſurgeon at Greenwich, when ſhe related to me the above complaints, now much augmented, having a ſenſe of fulneſs in that ſide (which was ready to burſt, as ſhe termed it) and an evident fluctuation in the right cavity of the thorax. But her left ſide was free from complaint. She made very little urine, and that limpid. The expectorant medicines (bliſter and cathartic) were adminiſtred without the leaſt relief; her ſymptoms gradually increaſing.

On the 1ſt of January 1763, ſhe could breathe in no other ſituation than that of the thorax brought forward to the knees, in which poſture ſhe continued till the 30th of January, when finding the ribs elevated exceedingly, and the right ſide of the thorax uniformly diſtended, with every other reaſon tending to confirm the notion of a fluid's being lodged there: we, in company with Mr. William Sharp (whoſe opinion we had, this day, requeſted) propoſed the operation to her, which the preſent preſſure of her diſeaſe, and the little probability of her living long in that ſtate, determined her to conſent to.

I, then, in preſence of Mr. William Sharp, ſurgeon to St. Bartholemew's, and Mr. Mills, made an inciſion, about four inches long, between the ſixth and ſeventh ribs, (reckoning upwards) and about half way between the ſpine and ſternum into the cavity of the thorax, and diſcharged from thence ſeven pints of limpid ſerum. Immediately the difficulty of breathing was removed, but a faintneſs ſucceeding ſeemed to endanger her

her for a short time, occasioned more by the sudden removal of the pressure from the lungs, than any other inconvenience from the operation, the loss of blood being very inconsiderable.

From this time to the next morning, the urine was secreted and discharged to the quantity of three pints more than she had drank. On the first dressing, the next day, there issued about a spoonfull of serum, but none afterwards: and though she remained weak and faint for several days, yet she had no other inconvenience, from the time of the operation to that of the cicatrization of the wound, which was compleated in less than a month; the wound having been dressed superficially the whole time.

It may be remarked, that, though, at the time of the operation, she was two months gone with child, she nevertheless compleated her pregnancy, and is now in as good a state as she had enjoyed for many years before.

XXXIX. *A Letter from Mr.* Emanuel Mendes da Coſta, *Librarian, &c. to the Royal Society, to* M. Maty, *M. D. Sec. R. S. containing a Supplement to the Account of the Diſcovery of Native Tin, Art.* VII.

Dear Sir,

Read Dec. 18, 1766.

I Communicate to you the following paragraph from a letter written to me by the Rev. William Borlaſe, L L. D. and F. R. S. of Ludgvan, near Penſance, in Cornwall, and bearing date November 23 laſt paſt. As it is a paragraph relative to the native tin found in Cornwall, which I had the honour to communicate to the Royal Society on the 6th of March laſt, and is ordered to be printed; permit me to beg of you to read it at the meeting to-morrow, that, if judged worthy by the Committee of papers, it may be printed with the former paper as a part of it.

I am, with great reſpect,

SIR,

your very obliged

humble ſervant,

Royal Society houſe,
December 17, 1766.

Emanuel Mendes da Coſta.

EXTRACT.

EXTRACT.

"Mr. Henry Rofewarne, of Truro, fays, that
"when he fent the firft fpecimen (prefented to the
"Royal Society by me William Borlafe, and now
"lodged in their Mufeum) he mentioned as a proof
"of its being native tin, that between the ore and the
"tin there was a mixture of quartz: but, upon a
"nearer examination and fome trials with aqua fortis,
"he and another perfon found it was not quartz. At
"laft, on melting a piece, he perceived no fmall
"quantity of arfenic to be mixed with it, and there-
"fore fufpected that the white parts which had
"paffed for quartz were nothing but arfenic. Accord-
"ingly he fcraped off a little of it and put it on a red
"hot iron, where it immediately caught fire, and
"evaporated into fmoke, leaving behind it the moft
"poifonous ftench they ever fmelt. This confirmed
"fome, who had hitherto doubted, in the moft
"firm belief that it really was native tin and genuine,
"it being impoffible for tin to be melted and the
"arfenic left untouched."

XL. *A Sup-*

XL. *A Supplement to the Account of an Amphibious Bipes; by* John Ellis, *Esq;* (*Art.* XXII.) *being the Anatomical Description of the said Animal, by Mr.* John Hunter, *F. R. S.*

Read June 5, 1766.

THE tongue is broad and has very little motion. It has a bone similar to that in birds, turtles, &c. On the posterior and lateral parts of the mouth, are three openings on each side; these are similar to the slits of the gills in fish, but the partitions do not resemble gills on their outer edges, for they have not the comb-like structure. Above * and close to the extremity of each of these openings externally, so many processes arise, the anterior the smallest, the posterior the largest; their anterior and inferior edges, and extremity are serrated, or formed into fimbriæ: these processes fold down and cover the slits externally, and would seem to answer the purposes of the comb-like part of the gill in fish.

At the root of the tongue, nearly as far back as these openings reach, the trachea begins much in the same manner as in birds. It passes backwards above the heart, and there divides into two branches, one going

* To avoid the confusion in our ideas, which might arise from the use of the words anterior, posterior, upper, lower, &c. in the whole of this description, the animal is considered in its natural horizontal position, so that the head is forwards, the back upwards, &c.

to each lobe of the lungs. The lungs are two long bags, one on each ſide, which begin juſt behind the heart, and paſs back through the whole length of the abdomen, nearly as far as the *anus*. They are largeſt in the middle, and honey-combed on the internal ſurface through their whole length. The heart conſiſts of one auricle and one ventricle. What anſwers to the inferior vena cava, paſſes forwards above, but in a ſulcus of the liver, and opens into a bag ſimilar to the pericardium; this bag ſurrounds the heart and aorta, as the pericardium does in other animals; from this there is an opening into a vein which lies above, and upon the left of the auricle, which vein ſeems to receive the blood from the lungs, gills, and head, is analogous to the ſuperior vena cava, and opens into the auricle which is upon the left of the ventricle. The aorta goes out, paſſing for a little way in a looſe ſpiral turn, then becomes ſtreight, where it ſeems to be muſcular; at this part the branches go off, between which there is a riſing within the area of the aorta like a bird's tongue, with its tip turned towards the heart*.

* This account of the venæ cavæ opening into the cavity of the pericardium may appear incredible; and it might be ſuppoſed, that, in the natural ſtate of the parts, there is a canal of commu-nication going from one cava to the other, which being broken or nipt through in the act of catching or killing the animal, would give the appearance above deſcribed. I can only ſay, that the appearances were what have been deſcribed, in three different ſubjects which I have diſſected; and in all of them the pericardium was full of coagulated blood. But, beſides the ſmallneſs of the ſubjects, it may be obſerved that they had been long preſerved in ſpirits, which made them more unfit for anatomical enquiries. They had been in my poſſeſſion above ſeven years.

The

The liver is principally one lobe, pretty cloſe to the heart at the fore part, and paſſes back on the right of the ſtomach and inteſtines; at its anterior extremity on the left ſide, there is a very ſhort lobe, ending abruptly. The gall-bladder lies in a fiſſure on the left ſide of the liver near its middle; there is no hepatic duct; the hepato-cyſtic ducts, which ſeem to be three in number, enter the gall-bladder at its anterior end or fundus, and the cyſtic duct paſſes out from the poſterior end of the gall-bladder, and terminates in the gut, about half an inch from the pylorus. The œſophagus, which is pretty large, paſſes back, and is continued into the ſtomach in the ſame line. The ſtomach, at the poſterior end, bends a little to the right, where it terminates in the pylorus. The inteſtines paſs back making many turns; at the poſterior end they become pretty ſtreight, forming what may be called the colon, or rectum, where they are a little larger and run to the anus in a ſtreight direction. At the beginning of this larger part of the inteſtinal tube, there is no valvular ſtructure. The ſpleen is a very ſmall but long body; its anterior end is attached to the upper ſurface of the ſtomach, and it is continued back along the left ſide of the meſentery, to which it adheres. The pancreas is a ſmall body lying above the duodenum, and is attached alſo to the left ſide of the meſentery. The kidneys are ſituated in the upper and poſterior part of the abdomen, having the rectum paſſing below and between them as in the ſnake, &c. Below the rectum lies a long bag, like a bladder; it adheres all along to the inſide of the abdominal muſcles, and its mouth opens into the rectum; but whe-

ther

ther it is the bladder of urine, or not, I cannot tell. On each ſide of the rectum, cloſe to the lungs, there is a body, the poſterior end of which reſts upon the anterior end of the kidney: whether they are teſticles or ovaria, I cannot pretend to determine; but ſhould imagine that they are either the one or the other.

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